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CLINICAL LECTURES ON SURGERY,

DELIVERED AT THE

HOTEL DIEU, PARIS,

BY

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[Revised, before translation, by the Baron himself.]

ON DISLOCATIONS OF THE SHOULDER.

DIAGNOSIS BETWEEN DISLOCATION AND FRACTURE.

A CELEBRATED writer, in speaking of dislocations of the humerus, has affirmed that there are few maladies, the history of which is more advanced, and in the treatment of which the surgeon approaches more closely to the ideality of perfection. In the course of this lecture, however, it will be easily perceived how unfounded was this assertion, especially at the period when it was made. In fact, many questions of deep practical importance were then either scarcely agitated, or still undecided. Modern authors agree thus far—in admitting that these luxations operate primitively in three directions, the most frequent of which is that from above downwards, and beneath the glenoid cavity. My experience has, indeed, confirmed this theory. Again, it was an established principle, that in orbicular articulations the dislocation was invariably *complete*. Many facts, however, establish, that in that of the scapulo-humeral articulation, the luxation sometimes is of the *incomplete* kind. The distinguishing symptoms of luxation and fracture of the superior extremity of the humerus were also considered obscure, but in a recent case the characteristic marks of these lesions have been pointed out with such precision, that error in this particular for the future will depend on ignorance alone. In numerous cases, too, of old dislocations, the ordinary method of reduction was insufficient even in the hands of the ablest operator. A mode, however, hitherto but little tried, has been put to the test in the Hotel Dieu, and with the most signal advantages. Lastly, the important question of determining the latest period for the reduction of luxations in general, and this in particular, has hitherto been almost untouched by writers in this department of surgery. Such are the chief points which will receive elucidation in the subsequent observations. We shall previously describe a case which will serve as an introductory text.

Case 1.—A female mendicant, ætat. 41, received at the Hotel Dieu on the 5th of last August, related, that on the 9th of the preceding month she had been roughly treated by some individuals, who threw her down and struck her with a stick. On getting up she exclaimed that her arm was put out of place. Some days afterwards, having been brought to Saint Lazarre, the medical attendants of that establishment thought she was right, and made four different attempts at the reduction, which did nothing but increase the torture of the patient. Indeed, after these fruitless extensions, intense pain was felt even in the arm and fingers. A month had elapsed before she came to the Hôtel Dieu, and she then presented the following symptoms:—projection of the acromion, flattening of the deltoid, the elbow separated from the trunk and insusceptible of being brought into juxtaposition, incapability of elevating the arm towards the head, and, lastly, a bony projection in the axilla. This reunion of symptoms was strongly characteristic of luxation, but they equally appertained to fracture. It is true that crepitation and the mobility of bony fragments, specially belonging to the latter, were not present, but the interval since the accident was sufficient to have occasioned the cessation of these phenomena had they even existed before. Besides, fracture might have been produced by the blows of the stick, as well as luxation by the position of the limb against the ground at the moment of the patient's fall. Lastly, the bony prominence in the axilla did not resemble at all the equal rotundity of the head of the humerus.

You see thus, Gentlemen, that if you only consult the rational symptoms established at present by all authors, without exception, as affording characteristic differences between fracture and luxation, it would be impossible to form an opinion *à priori* on the nature of the case before us. An attempt at reduction was nevertheless decided on, care being taken to avoid the injury the patient would sustain if the case was absolutely a fracture. For we must not forget, that where fracture is mistaken for luxation, it may, it is true, be reduced, but when the parts are subsequently left to themselves, the muscles gradually reproduce the displacement. Before proceeding to the operation, the patient was prepared, as is our custom, in all old dislocations, by venesection, the application of poultices around the articulation, by baths, and moderate doses of the watery extract of opium. On the day appointed, the reduction was attempted in the manner we will subsequently describe. At the first efforts of extension the patient uttered loud cries. To divert her attention, she was accused of having been engaged in a thieving excursion on the night of the accident, a reproach which succeeded in eliciting bitter reclamations, but failed in facilitating the reduction. Several other attempts were equally unsuccessful. The deltoid, however, seemed to be less flattened, a circumstance which, coupled with the useless efforts at reduction, lent additional probability to the hypothesis of fracture. The diminished flattening of the deltoid might result from a slight displacement, externally, of the yet tender callus. In order to increase this tendency, a large cushion was placed between the arm and the trunk, and the elbow was drawn as closely as possible to the side by means of a bandage. But at the end of four days this treatment had produced no effect.

The facts thus related give rise to three important questions, of which it is urgent to find the solution, 1st. The symptoms we described being completely inadequate, by what means can we arrive at a true and certain diagnosis? 2nd. Supposing the case was luxation and fracture; the mode of reduction we constantly employ with success having been vainly tried in this instance, by what proceeding could we succeed? 3rd. Does the interval which has elapsed since the accident, contra-indicate the operation? Or, in generic terms, up to what period is it possible to reduce a luxation? In consequence of the difficulties we met with, it became imperative on me to study minutely the dispositions of the joint in this patient. At the same time a young surgeon, Dr. Malgaigne, had the kindness to offer to impart to us the ideas which the case suggested to him. I received them with eagerness, since they seemed to me to be just in the extreme. The following are the results of this double investigation:—In the first place there existed in this patient a considerable elongation of the injured limb. But in every fracture of the long bones, if there be no displacement, the member preserves its natural length, and if there be displacement, it becomes shortened. In this female, the luxated limb, measured from the projection of the acromion to that of the olecranon, or to one of the humeral condyles, was longer by half an inch than the arm at the other side. This single symptom I consider irrefragable evidence of the existence of luxation. But besides this, M. Malgaigne points out others to which I proceed to allude. The first, which is indeed an effect of that I have just described, consists in an augmentation in the depth of the anterior wall of the axilla, and indeed in the present case, when measured from the inferior edge of the clavicle, to the free anterior edge of the axillary fossa, the axillary wall of the affected side was greater by half an inch than at the opposite side. Secondly, according to this gentleman the head of the dislocated bone should necessarily form an anterior projection in the spot where the subclavicular hollow is usually remarked. The difference of aspect thus produced is especially sensible in thin people, and in the case under discussion was very apparent. Lastly, M. Malgaigne asserts, that on pressing the fingers immediately below the acromion, the deltoid muscle is readily depressed in case of luxation; and that was precisely what was observed in this woman. These four signs, ever constant in luxation, ever absent in fracture, each dependant on the other, and, even though isolated, of sufficient conclusiveness, were demonstrated in the amphitheatre.

NEW AND IMPORTANT MODE OF REDUCTION.

The nature of the lesion being thus precisely ascertained, it was essential to learn whether or no the long duration of the accident contra-indicated the reduction. Experience, and numerous facts, having long since convinced me that injuries of this kind, and even of much longer standing, may be safely submitted to operation, we did not hesitate in our decision. The next point was, consequently, the choice of the method to be pursued, in place of that which had failed at Saint Lazarre and the Hotel Dieu, and M. Malgaigne availed himself of the opportunity to propose a plan hitherto untried in France. It consists in making extension, the arm being lifted up forcibly, and consequently shortened, instead of ex-

tending the limb in a depressed and elongated position.*

In order to give the new method a trial, the patient having been suitably prepared, the operation was proceeded with on the 14th of August by Dr. Malgaigne himself. The patient having been placed in the recumbent posture, a folded cloth was placed in the acromion, its two ends brought towards the feet, and held at first by assistants, but afterwards secured in the ring, in consequence of the struggles of the patient. The extensor loop being arranged as usual, was entrusted to two assistants, and the dislocated arm lifted up as high as possible, so as to render it almost parallel with the axis of the trunk. Extension was practised at the same time, the forearm having been pronated, in order to substitute a straight lever for the bent lever represented by the limb in a state of supination. The first extensions only appeared to cause trifling pain. The head of the bone appeared perfectly in the hollow of the axilla, which it filled up. Gradually it elevated itself towards the cavity whither the extension drew it, and the two walls of the axilla, previously effaced, now began to disclose the cavity which separates them in the normal state. Meanwhile pressure was made with the fingers and palm of the hand on the head of the bone, in order to assist it in regaining its cavity, to the level of which it had arrived. Twice in this expectation the arm was brought towards the trunk, but twice did the bone refuse to enter. Extension having again been repeated, the hand was pressed vigorously on the dislocated head of the humerus; the assistants then depressed the arm, and brought it close to the trunk, still persevering in the extension. A first trial failed, but the second completely succeeded, and the bone returned to its socket without noise. The shoulder had now regained its roundness, the elbow readily approached the trunk, the movements of the articulation were performed with facility, and the projection of the axilla disappeared. On repeating the measurement of the arm, it still remained longer than the other, and the projection of the head of the humerus below the acromion process, seemed a little less prominent than natural. It is not to be lost sight of, that the head of the bone was pressed upwards and downwards with unusual vigour, as the attempt at its reduction succeeded.

Remarks.

In a few days this patient's cure was completed, the arm remaining still a little longer than natural. Here then is our first success, publicly obtained, at the Hôtel Dieu, in a most difficult case, and by the new method. But whence did it happen that when the reduction was effected, the arm did not regain its natural length? Could it be that a portion of the capsule got entangled in the glenoid cavity, or did there exist any enlargement of the cartilages? The latter presumption is not perhaps unfounded. Two causes indeed may determine the enlargement or swelling of the cartilages of an articular cavity after a luxation. In the first place, the cause which occasioned the original displacement must necessarily have operated with more or less violence on the cavity, by means of the head of the bone. Thence is a source of irritation, of inflammation even, the chronic effects of which you know to be the increase of the density and thickness of the parts it affects. Moreover, experience has repeatedly shown that articular cavities, after the separation of the bone which originally occupied them, tend continually to close, and, indeed, become totally

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* It was an amazing, and, at the same time, a gratifying sight to see this young gentleman stand up in a crowded amphitheatre, and volunteer his suggestions to the veteran of the Hôtel Dieu. But from the ability M. Malgaigne displayed, professor and pupils listened to his observations with the utmost attention. So far from being annoyed at what our hospital surgeons would have deemed an unpardonable piece of impudence, M. Dupuytren testified his approbation of the young surgeon's frankness, by permitting him to carry his own proposal into practice before the pupils. M. Malgaigne stated in his address to the class, that the anatomy and pathology of this kind of luxation, had led him to adopt the method before he was acquainted with the observations of Mothe, who had employed it before, but solely in old dislocations.—*Translat or's note.*

With reference to M. Malgaigne's allusion to Mothe, the French reporter of M. Dupuytren's lectures offers us the following interesting statement. In 1785, Mothe, one of the surgeons of the Hôtel Dieu of Lyons, sent to the Academy of Surgery a memoir on the luxations of the humerus, in which he proposed a new method, of which he said he was the inventor. It consists in seizing the arm at its lower extremity, close to the elbow, the patient being seated, or placed on a bed, with the forearm bent, and in lifting up the arm gradually, at the same time the operator drawing it towards himself—that is to say, practising extension till the limb is as near the head as possible, or in a line parallel with the axis of the trunk. The arm in this position is extended still further, and counter pressure practised on the shoulder. Mothe reports eight cases of luxation thus reduced, one of which was of five weeks' standing. In Germany, and several northern nations, this method is generally known by the name of Mothe's operation, and has become quite popular. Rust, the eminent surgeon of Berlin, gives a description in his journal, the *Magazin fur die gesammte Heilkunde*, t. 10. p. 164.

effaced after an uncertain lapse of time. Elongations, therefore, of dislocated limbs, as in the case before us, and in others of the hip-joint, may readily be explained on this supposition. Nevertheless, the explanation afforded by M. Malgaigne is equally satisfactory. According to this gentleman, in recent luxations, the supra and infra spinous muscles are applied against the glenoid cavity; the subscapular muscle forms a kind of cap on the dislocated head. Is it not, therefore, likely that the time elapsed has permitted the formation of adhesions, which would, moreover, have been favoured by the irritation produced in the numerous vain efforts at reduction? Sometimes there have been found, on the dissection of such old dislocations, false membranes approaching even to the cartilaginous state. Moreover, the swelling in the vicinity of the articulation could not, of course, cease instantaneously on the reduction, and, doubtless, the swelling is not less at the superior part than at the other sides of the cavity. Perhaps, indeed, this elongation, after the operation, only appears extraordinary because not previously noticed by authors. Such is an epitome of M. Malgaigne's reasoning respecting this phenomenon. Further researches are required to elucidate the nature of this elongation of the limb.

Having been inclined to believe that the elongation was an attribute only of the dislocation downwards below the glenoid cavity, while M. Malgaigne, on the contrary, thought that the head of the humerus occupying the cavity of a vault formed by the acromion, the coracoid process and the uniting ligament should manifestly be on an inferior level when it is pushed beneath either of the pillars of this vault, an appeal was made to experiment, in order to decide the question. With the dry bones it was evident the subacromial dislocation induced the elongation of the arm, but in the case of the subcoracoid luxation, the matter still remained doubtful. Experiment was again resorted to. This luxation was produced on an articulation newly prepared, with its ligaments entire, and all accurately measured. Elongation to the extent of half an inch was found to have been occasioned. As to the dislocation downwards, the experiment was made with the same articulation. All the muscles having been removed, it was nevertheless impossible to effect the displacement, until the ligamentous fibres which unite the capsule to the acromion and coracoid apophysis, were divided with a bistoury. Elongation to the enormous extent of an inch and a half was produced, an extent greater than has yet been noticed in the living subject. But the question naturally suggests itself,—Do the results thus obtained on the dead subject take place in the same manner in the living? The answer can only be obtained by numerous autopsies.

Farther Trials of the New Method.

The extent to which I have treated of the preceding case, sufficiently shows the many interesting questions which the study of scapulo-humeral luxations still presents for our examination. Since this case, two others have presented themselves, the reduction of which was proceeded with in the new method.

Cases 2 & 3.—The first of these was a washerwoman, ætat. 67, of low stature, thin, and of feeble muscles. She had fallen down the stairs of a cellar, and had rolled along perhaps a dozen steps. The accident occurred at eight a.m., on the 27th last October, and she immediately repaired to the Hôtel Dieu, where it was ascertained that she presented a *sub-coracoid* or *inferior* and *anterior* luxation. The reduction was immediately proceeded with. An assistant seized the hand of the dislocated limb, lifted it up parallel to the axis of the body, and drew it directly upwards. A second assistant leaned on the scapula, in order to produce counter-extension, while I directed the head of the humerus with both thumbs. At the first effort of extension the reduction took place without difficulty, and with scarcely the least pain. The arm was then brought down cautiously, close to the trunk, to which it was secured by a bandage. In twelve days this patient was cured.

This method consisting, as it were, in lifting up the patient by the dislocated arm, extension cannot be practised in the sitting posture, if the patient be of high stature. In such case the injured person must be laid in the horizontal attitude, or the assistant who makes the extension must mount on a table. The morning after the preceding operation, a female, aged 45, of weak muscle, but of high stature, came to the consultation, presenting also a sub-coracoid luxation. She was placed on her back, a cloth folded like a cravat passed over the shoulder, both ends turned over and brought downwards towards the trunk on the opposite side, and confided to two assistants, in order to practise the necessary counter-extension. Two other assistants lifted up the arm parallel to the axis of the trunk, and practised extension, while the head of the humerus was pushed from below upwards with both thumbs. At the first effort the luxation was reduced without pain. The patient immediately began to laugh, and left the hospital.

In these two cases it is true the circumstances were all favourable. We had only to manage recent luxations, in emaciated

women, advanced in years, and retaining no muscular energy. In similar cases the ordinary method would doubtless have succeeded. Nevertheless we must remark, that in these cases, and with the new proceeding, it was unnecessary to take any precaution, or to fix the patients to the ring. The reduction was operated without effort, and with an ease and promptitude truly remarkable. It would, I think, be unjust to attribute all these advantages to the age and constitution of the patients, and to refuse to the operation the share of praise it so justly deserved.

We have thus, Gentlemen, dwelt with sufficient length on a method hitherto unknown to you, and the value of which you were enabled to estimate by its practical application. I propose to employ it in other cases according as they present themselves. In the meanwhile we shall direct your attention to some other and not less important questions connected with the scapulo-humeral luxations.

Liability to Scapulo-Humeral Luxations.

Even though the records of clinical surgery were silent on this subject, the anatomical disposition of the scapulo-humeral articulation would have been sufficient to indicate its liability to the accident. It is indeed only at the sacrifice of strength that this joint possesses its extraordinary mobility. A cavity protected by an osseo-fibrous vault, a bony head retained in this cavity, a capsule which envelops and maintains the relations of the entire, a great number of muscles for the motion of the parts—such, you are aware, constitute the elements of this articulation. But the disproportion which exists between the dimensions of the head of the humerus and those of the glenoid cavity,—the laxity and tenuity of the capsular ligament, especially at its inferior edge, the situation and uses of the arm, exposed perpetually to the action of external violence—all these are circumstances which provoke and favour the displacement of the limb, and they would act more powerfully still, did not the scapula accompany the shoulder in its movements, and thus extend the limits within which the bones of the articulation preserve their normal relations.

Directions of the various Luxations.

There is, perhaps, no subject in surgery respecting which authors differ so much in opinion as regarding the different species of luxations of the shoulder. This, however, is not the place to give the history of these divergencies of opinion. I shall confine myself to stating to you, that experience and observation have taught me that the arm may be primitively displaced in three principal directions: 1st, Below; on the *axillary border* of the scapula; 2nd, Within, or anteriorly, into the *sub-clavicular* fossa; 3rd, Before, or without, into the *sub-spinar* fossa. The presence of the acromion and coracoid processes, united by a very strong ligament, and the situation of the humeral extremity of the clavicle, directly oppose the luxation *upwards*; nevertheless Sir Astley Cooper admits a partial luxation in this last position. The upper part of the capsule being torn, the head of the bone, according to this author, applies itself against the posterior edge of the coracoid process.

Luxation Downwards.

The luxation of the humerus *downwards*, the only one primitively possible according to many authors, is unquestionably that most frequently met with. It is generally produced by a fall on the elbow, and especially on the palm of the hand, while the arm is extended and separated from the body. The humerus then inclines towards the glenoid cavity so as to form with it an acute angle, the sinus of which is turned upwards. The head of the bone gliding thus from above downwards on the articular cavity, is strongly pushed against the inferior part of the capsule. This pressed against inversely by the weight of the body, tears asunder, and permits the escape of, the head of the humerus, which is, moreover, hurried on by the contractions of the greater pectoral and trapezius muscles, and is at length brought to the inner side of the anterior edge of the scapulum, between the subscapular muscle and the long portion of the triceps. The great pectoral and trapezius muscles act as a lever, the fulcrum of which is the elbow, the resistance being at the articulation of the shoulder. The luxation *downwards*, again, according to several authors, may be produced by a violent blow on the external part of the shoulder, below the acromion, but then the luxation is often complicated with fracture of the scapula or humerus. The displacement may also be determined by the muscular contractions alone, in a violent effort at elevation of the arm in lifting up a burthen, or during an epileptic paroxysm, whether we admit that the deltoid depresses the head of the bone, and pushes it, inferiorly, out of the capsule, or think with M. Boyer, that there is a simultaneous contraction of the pectoralis major, trapezius, and levator muscles.

The symptoms of this dislocation are, the elongation of the arm, its oblique direction outwards, the elbow apart from the body, and incapable of being brought close to it, the head and the trunk bent towards the affected side, the fore-arm in a state of demi-

flexion, incapacity of voluntary elevation or rotation, acute pain occasioned by any attempt at these movements, deformity of the shoulder, and considerable projection of the acromion, below which exists a hollow occasioned by the flattening of the deltoid, no longer sustained by the head of the humerus. Lastly, this forms in the axilla a hard and rounded tumour.

Luxation within.

The dislocation *within*, or anteriorly, occurs in a fall on the elbow while held apart from the trunk and before the body. The persistance in this attitude when the patient gets up, the presence of a tumour formed by the head of the humerus below the clavicle, and before the summit of the shoulder, which is less deformed than in the preceding variety. Finally, the impossibility of bringing the elbow forwards without causing intense pain, leaves no doubt of the existence of a dislocation of this description. It is, however, much more rare than the luxation downwards. It is as rarely primitive, and is generally consecutive to the first variety.

Luxation without and before.

A fall on the elbow while held upwards, may produce the luxation *without* and *before*. This displacement is extremely unusual, and would, perhaps, be impossible without a previously defective construction of the glenoid cavity, when, for example, it is abnormally inclined forwards, and considerably elongated. In this luxation the arm, little distant from the breast, is directed forwards and inwards, the shoulder is merely flattened at its anterior part, and the head of the humerus projects below the spine of the scapula, and towards the external side of the anterior angle of this bone.

Epitome of Opinions.

I have thought it necessary to enter into these elementary details, in the first place, for the information of many of those present; but, above all, in order to collate them with the modifications which it is desired to introduce into the theories generally admitted, and which I proceed to epitomize. 1st. The elongation of the limb in the luxations *downwards* is not a newly observed fact, I have observed it in all the cases I have treated in my long experience. But does this elongation exist in every description of luxation of the shoulder, as some persons assert, or, on the other hand, is the arm sometimes longer and sometimes shorter, according to the variety of the displacement, as some other persons imagine? 2nd. Can the humerus be dislocated primitively only in one direction, and are the other luxations merely consecutive on this? 3rd. If it be true that there exists but one primitive luxation, in what direction does it take place? 4th. Can luxation not occur without laceration of the capsular ligament, or is it sufficient for the escape of the bone that the ligament should be merely dragged or extended. 5th. This torn capsule in exercising a constriction about the displaced bone, can it oppose the reduction as Desault believed, or is this obstacle as imaginary as Sir Astley Cooper persuades us? 6th. There still exist varieties of opinion even regarding the anatomical structure of some of the uniting media of the articulation, and each author explains himself after his own manner of viewing the structure of the parts. Thus it is that some in whose opinions I have all along coincided, admitting that the lower part of the capsule is its weakest side, have considered the luxation *downwards* as the most frequent, while others maintain that this lower part is the thickest, that the capsule is slighter elsewhere, and only place the luxation *downwards*, in the third rank, for frequency and facility of production. Now in the recollections of my experience I could readily find sufficient facts to decide the majority of the preceding questions, but this discussion would prove too extended at present. I confine myself now to point out the questions to your attention, and as new facts present themselves we shall return to their consideration.

Incomplete Luxation of Orbicular Joints.

It is generally said, that in orbicular joints dislocation is invariably *complete*. Pathological anatomy has, however, proved the contrary, in offering us examples of *incomplete* dislocations of the arm and thigh. In 1824 the surgeon of one of the Parisian hospitals presented to the Academy a preparation of the articulation of a man who died eight months after dislocation of the humerus, which had never been reduced. It presented a false articulation, constituted in one part by the glenoid cavity of the scapula, and a small portion of the surface of the ribs; in the other, by the head of the humerus, hollowed into a groove to receive the anterior border of the glenoid cavity, in a species of ginglymoid cavity. During life the arm could perform very limited motions from before backwards. In a case of spontaneous luxation of the femur, the same practitioner has seen the softened head stop short on the anterior ridge of the cotyloid cavity, and there remain fixed as if on a rivet. Here, then, are two examples of the *incomplete* luxation of the two principal orbicular joints; a luxation difficult indeed to be imagined, and denied universally by writers on the subject.

We have already seen what are the symptoms peculiar to each species of scapulo-humeral luxation; but we have also pointed out that these symptoms equally appertain to fracture of the superior extremity of the shaft of the bone. The distinguishing marks were first pointed out in the clinical lectures at the Hôtel Dieu, and have since become the object of an excellent memoir published by Dr. Marx.

Luxation and fracture, simultaneously and otherwise.

Every person who is affected with a luxation or fracture of the superior part of the humerus, must have fallen on the side of the body corresponding to the lesion, but the position of the limb at the moment of the fall is not the same in both cases; and this difference commonly decides on the consecutive injury, and enables the surgeon to ascertain its nature. If any displacement occur when the arm separated from the body has been held *forward* and *outward* to meet the ground and deaden the effects of a fall, this displacement will be a luxation without fracture. If, on the contrary, the arm have been retained close to the chest as in an unforeseen and sudden fall, the patient, for example, having his hand in his breeches pocket, it is on the tip of the shoulder that the weight of the body is projected, and then, when there is displacement, it is the effect of a fracture or comminution of the head or upper part of the bone. In both cases there is acute pain in the tip of the shoulder, and the patient always believes that he fell on the pained part. But when the pain is caused by luxation, the fall having been on the palm of the hand, this is usually soiled with dust or mire, or presents ecchymosis or excoriations. If, on the contrary, the pain is determined by the fracture of the bone, it is ascertained, that the fall took effect on the shoulder by the absence of all marks on the hand, by the soiling of the clothes or of the skin of the shoulder, or by the bruises, ecchymosis, or wounds of this region. In the dislocation, again, the pain depends on the laceration of the fibrous capsule and adjoining tissues. In fracture it is produced by the contusion of the shoulder, and the tearing of the soft parts by the lower fragment of the bones. As an effect of these lesions, ecchymoses may, and usually do, take place; but as this ecchymosis in dislocation is produced by laceration of the internal parts of the joint, while in fracture it arises from external contusion, the seat of the extravasation is altogether different in both cases. In dislocation it is situated at the anterior or inner part of the arm; in fracture, on the upper surface of the shoulder. Lastly, it is rather rare in luxation but constantly observed in cases of fracture.

In both these lesions the acromion is projecting, the deltoid flattened. A hollow is felt at its internal side, and a projection in the hollow of the axilla; but an exact analysis of these symptoms almost invariably removes the doubts which a superficial examination occasions. In fact, the prominence of the acromion is more considerable, the flattening of the deltoid greater in the luxation than in the fracture, in which this muscle appears shortened and swelled. The hollow beside the deltoid is greater also in luxation, the prominence in the axilla more decided and rounded. These are, to be sure, differences only in degree dependent on the completeness or incompleteness of the displacement of the bone.

In luxation there is no mobility of the limb or crepitation, but both these phenomena are readily perceptible in fracture. Is the humerus dislocated? You may try to move the limb as long as you please, but the articulation acts as a continuous piece, and the scapula often yields as if one and the same bone with the humerus. Is the arm fractured, on the other hand? There is preternatural mobility at a certain point of the upper extremity of the bone, usually accompanied by crepitation, most readily perceptible when the arm is rotated on its axis. Finally, what chiefly distinguishes luxation from fracture is, that the first requires greater efforts to be reduced, and that it suffices after reduction to maintain the arm close to the chest; while in fracture an apparatus is indispensable to preserve the fragments in contact, to prevent the renewal of the displacement by the action of the muscles, and to obtain a cure without deformity or embarrassed facilities of motion.

It sometimes occurs that when fracture consists of a simple solution of continuity without displacement, it is confounded with a severe contusion of the tip of the shoulder. The crepitation and preternatural mobility of the parts can alone decide the difficulty. We must not, however, be deceived by a spurious crackling which is frequently met with in violent contusions of the shoulder, and which is occasioned by inflammation of the articular surface and diminished secretion of synovia. The following cases illustrate these observations.

FRACTURE OF THE NECK OF THE HUMERUS, WITH SLIGHT DISPLACEMENT OF THE HEAD OF THE BONE, SIMULATING LUXATION.

A shoemaker, æt. 60, while walking on an inclined plane, received a fall in which the weight of the body bore on the left arm while applied to the side. Brought to the

Hôtel Dieu next morning the patient presented a considerable swelling of the scapulo-humeral articulation, a shortening of the deltoid muscle, an augmentation of thickness and breadth of this muscle which could also be slightly depressed, a projection of the acromion more marked than in ordinary cases, the impossibility of bringing the arm close to the trunk, obscure crepitation and motion of fragments, a rounded tumour resembling the head of the humerus in the hollow of the axilla, a kind of projection at the inner part of the shoulder under the tendon of the great pectoral muscle. By this collection of symptoms the difficulty of the diagnosis may be estimated. On the supposition that there was fracture the apparatus was employed. But in two days the swelling increased, and in dressing the patient it was perceived that the deltoid muscle was less wide, thick, and shortened, than it at first appeared; that it permitted its own depression; that there was a hollow below the acromion, which, was itself sufficiently prominent; finally, the absence of the least mobility of fragments, and the perfectly rounded form of the axillary tumour. Some hesitation respecting the first diagnosis was accordingly created. The limb was then subjected to some extension. A cushion filling the axilla was adjusted as in fractured clavicle. The arm applied to the cushion was fixed to it by several turns of a bandage, which, strongly tightened, passed from the inferior extremity of the limb round the trunk, and acted so that the lower third of the humerus, covered by the bandage, was urged a little forwards and inwards, while its superior extremity was directed a little upwards and outwards, and reposed on the cushion. Five days after, the swelling diminished, indeed almost altogether disappeared. Crepitation was easily heard and felt; and the fingers introduced into the hollow of the axilla met the inferior fragment, which presented many inequalities of surface, and appeared to be composed of numerous and slightly movable pieces. There could no longer, of course, exist any doubt about the propriety of the original diagnosis. The head of the humerus could also be felt slightly displaced, and carried a little forwards and inwards. The apparatus was then re-adjusted, and at first renewed every third day. On the fortieth day it was finally taken off. There was no more mobility or crepitation, the limb had regained its ordinary length, and the deltoid muscle and acromion their natural condition.

LUXATION OF THE RIGHT HUMERUS UPWARDS AND FORWARDS, CONSECUTIVE ON A LUXATION INWARDS.

Hamlin, æt. 26, a crystal-cutter, while giving assistance during a fire, fell from the roof of a five-storied house into the court-yard, breaking through a wooden shed in his fall. Conducted to the Hôtel Dieu, he presented a dislocation of the humerus, and several violent contusions. The arm was placed on a pillow. Resolvents were applied on the contused places, and venesection was performed. Next day the patient lay on his back, the arm stretched on the pillow, separated from the body so as to form with it a right angle, and projecting inwards and upwards, the palm of the hand was turned forwards, and all the limb in the highest state of supination. When the hollow of the axilla was touched no projection was detected, but by moving the hand further on a projection formed by the head of the humerus was felt within and beneath the pectoral muscles, and only separated from the clavicle by a few lines. When the arm was urged towards the body, the patient being in a sitting posture, the prominence of the acromion near the shoulder was remarked. The deltoid muscle was also slightly flattened. Intense pain was occasioned by the attempts to move the limb. Beneath the clavicle was a rounded tumour elevating the pectoral muscles. Lastly, all voluntary movements of the limb were abolished. By these symptoms, luxation *forwards* and *upwards*, consecutive on luxation inwards, was easily recognised. Another bleeding was resorted to. Before the operation, which was performed next day, it was evident that it would prove very laborious from the strength and muscularity of the man, independently of the peculiar difficulties in the reduction of this variety of dislocations. And so it turned out, the reduction only having been accomplished after powerful and long-continued extensions, and by diverting the attention of the patient, by pressing and multiplied questions. The arm was then placed and maintained in a semiflexed posture, and bound to the trunk with a napkin. Emollient applications to the shoulder. Twenty days afterwards the patient was permitted to move the limb, but he was long before he regained its perfect use.

Sometimes the dislocation of the shoulder is complicated with fracture of its cervix or neck. In such a case we have to deal with an affection which baffles alike the efforts of nature and the skill of science. Despite of this impotence, however, good diagnosis is advantageous in directing us to the choice of the most suitable apparatus.

DISLOCATION COMPLICATED WITH FRACTURE.

A stout man, æt. 43, while climbing a ladder, badly fixed against a door, fell into the area along with the ladder. His narrative was too vague to give us precise no-

tions of what took place during the fall; he only recollected that the left arm passed between two rongs, that one of the spars of the latter pressed the tip of the shoulder with violence from without inwards; that at the instant he experienced dreadful pain in the scapulo-humeral articulation, and that he fell on the left side. Let us bear in mind, then, that the double lesion of the arm preceded the fall, since this took place on the opposite side, and since the patient actually felt the tearing of the shoulder.

A village surgeon was called in, who, believing he had to treat a simple luxation, practised numerous extensions, and then applied the ordinary bandage. It is worthy of remark, that during the efforts of extension the patient experienced great relief. Was not this a proof that the extremities of the bones having been replaced, ceased to irritate the surrounding soft parts? After twenty days absolute rest, the patient perceived that his shoulder was deformed, that his arm was longer than natural, and its movements intensely painful. Nevertheless he sought no better advice until the 25th of January, fifty-five days after the accident, when we found him in the following condition. The length of the left arm exceeded that of the right by an inch. The movements forward, very limited and dreadfully painful. Although the limb was almost parallel to the thorax, it could not be applied to it without exciting acute pain in the superior and anterior part of the shoulder. The movements of adduction were equally restricted and tormenting. Instead of a uniform and resisting convexity, the tip of the shoulder was sensibly flattened, though not in the same manner as in simple dislocation. The acromion projected. Both to the touch and sight the abnormal direction of the humerus was apparent. Instead of terminating under the acromial vault, it tended inwards to unite with the superior fragment, forming with the lower extremity of this fragment a projecting angle in the hollow of the axilla. Immediately below the acromial extremity of the clavicle, the head of the humerus was felt in remarkable relief, and elevating the deltoidean border of the great pectoral muscle. When slight pressure was made with the thumb from above downwards on this eminence, the movement produced extended to the entire arm.

The house pupil of the ward on the first day made some useless attempts at reduction, and then baths and cataplasms contributed to lessen the pain. However, all hope of reduction was vain. The sole rule of conduct to be observed in this melancholy complication, consists in effectually opposing the *inward* tendency of the ends of the fragments. This important precept is, moreover, never to be neglected. By suitable manipulation, first restore the limb to its natural form and length; then return to the patient in seven or eight hours; if you find the shoulder deformed, be assured you have a fracture to deal with.

We have seen by one of the preceding cases how difficult it is to distinguish even recent luxation from fracture of the humerus. Nevertheless, error in this respect is only common among ignorant or inexperienced persons. It has frequently happened, that we have received into our wards individuals labouring under fractures or luxations, which have been mistaken in town, despite the most characteristic symptoms. But in old luxations all these difficulties are of course much increased. You have seen this in the first case mentioned to-day. The symptoms peculiar to fracture may have disappeared if they ever existed. Those common to both affections remain, together with those peculiar to luxation. For these, then, must we diligently seek in perplexing and doubtful cases. We have already dwelt on the elongation of the arm, and of the anterior wall of the axilla, the deformity of the tip of the shoulder, and the ready depression of the deltoid by the fingers. As to the projection found anteriorly below the coracoid apophysis and great pectoral muscle, and which is independent of the osseous prominence in the axilla, more reliance is not to be placed on this than it actually deserves; for it is equally seen in cases where the fracture is accompanied by a trifling displacement of the bone, as the case of the shoemaker sufficiently established.

What is the longest period at which Luxations can be reduced?

We pass now to the consideration of a new question, to which attention was called for the first time in these clinical lectures, viz., What is the longest period after the accident when these luxations can be reduced?

The difference depending on the age of the luxation, is of the greatest practical importance; for the reduction of dislocations of many days standing is a very different thing from that in recent cases. The soft parts and the bone itself have acquired a certain habitude of position. The ligaments and muscles surrounding a deranged articulation, acquire a stiffness which yields with difficulty to the efforts at reduction. The laceration of the orbicular ligaments may have been *cicatrized*, so as to render impossible the return of the bone into its cavity. It is only by the multiplication of facts that we can arrive at the resolution of this question, and be enabled to trace out

rules for the guidance of the practitioner. Older writers thought, that after a few days, reduction should not be attempted, and this opinion was long maintained. Benjamin Bell believed so, although he knew the successes obtained by White and Freck. The authority of Bell long influenced Desault; but if Bichat be correct in his statements, experience altered Desault's opinion, and some successes obtained in cases from fifteen to twenty days old, induced him to try one at thirty-five. Bichat even stated, that during the two years preceding his death he had seen reduced, luxations of two, three, and even four months standing. A memoir containing six cases published in the *Repertory of Anatomy and Surgery* by M. Flaubert, surgeon of the Hôtel Dieu at Rouen, would be little calculated to encourage surgeons to practise on old dislocations. In five of these cases the operation occasioned terrible consequences, such as the laceration of large arteries, nerves, and muscles. The effects M. Flaubert states, are the more to be feared in proportion to the age of the luxation, and the degree of swelling and inflammation by which it is accompanied, and which appear to dispose materially to the laceration of the parts, by causing the adhesion of the nerves and vessels to the neighbouring parts. Moreover, the inflammation he states seems to soften the muscular and arterial tissue. The existence of this inflammation was demonstrated by the autopsies of two of the patients whose cases he described. In the sixth patient, the reduction of an old luxation of the thigh was followed by death, occasioned by violent local inflammation. From four of these cases M. Flaubert concluded that the paralysis often attributed to the luxation, is caused by the reduction itself.

M. Marx, a distinguished surgeon, to whom science is already deeply indebted, has treated of this subject with remarkable talent in the work already mentioned, and, from facts collected at the Hôtel Dieu, Paris, has arrived at results totally opposed to those of M. Flaubert. On what does this diversity depend—on differences of age, sex, or constitution? Evidently not. On the mode of reduction? This cannot be, for the good reason that the same method was followed in both hospitals. We can only say, then, that M. Flaubert has been more unfortunate than other surgeons. And, moreover, to quote the words of Dr. Marx, "If violent extension produce occasional mischief in old dislocations, the reduction of recent cases is not always exempt from this consequence." The history of 33 cases of luxation bears us out in our opposition to M. Flaubert. Of these 25 were of the shoulder, five of the femur, and three of the forearm. Of the 33,

5	were reduced from the	5th to the 10th day.
6		10th 20th
4		20th 30th
5		30th 40th
5		40th 50th
2		50th 60th
0		60th 70th
2		70th 80th
2		80th 90th
1		90th 100th
1		after two years.
33		

Twenty-six of these cases occurred in my hospital or private practice, the most recent was of 5, the oldest of 82 days standing; the majority were of 20, 40, and 50 days. One died, not of the operation, but of wounds of the soft parts received when the dislocation occurred. A second experienced some difficulty of moving the hand and fingers. This was an eight-day case. Lastly, a child, ætat. 10, with a dislocation of the elbow of 76 days, was not cured, and a unit is consequently to be subtracted from the preceding table. All the other patients were promptly and perfectly cured. The 27th and 28th cases belong to Desault. One, scapulo-humeral was reduced the 45th day, emphysema of the chest succeeded, but the patient, æt. 60, was effectually cured. The second scapulo-humeral also was reduced by Desault on the 90th day. It was a female, æt. 34, and she was perfectly cured in nine weeks. The 29th and 30th cases are described by Mothe. The 31st was in the case of a woman æt. 55, reduced by M. Sanson at the Hôtel Dieu on the 92nd day after only two attempts, and was perfectly successful. The 32nd is related in Delamotte's Treatise on Surgery. It had existed two months. Lastly, the 33rd is extracted from the *Mémoires de l'Académie de Chirurgie, tom. 5*, which contains the history of a luxation of the thigh, which, after various accidents, written consultations, and alternate recognitions and denials, was finally reduced after two years. In this case the patient was a young lady, æt. 22, who dislocated her thigh by a voluntary movement during her accouchement! Louis Forestier, an able surgeon of the town of Saint Claude, Tissot of Lausanne, and Cabanis of Geneva, as well as M. Guyenot of Paris, all recognised the luxation. Nevertheless, the fact is so extraordinary as still to tolerate some scepticism about its nature, and to prevent our receiving it as a precedent for practice. In fact, a young man whose thigh had been dislocated for two years, presented himself

at the Hôtel Dieu in June 1829, and was strongly advised to permit no attempts at the reduction.

Among the numerous cases of old luxations which have occurred in our practice, there was one presenting such extremely curious, and perhaps unparalleled, phenomena, that we shall relate it in this place.

Case. On the 4th July, 1829, an old woman presented herself at the public consultation, having a luxation of the upper extremity of the humerus. The accident was of six weeks standing, and the luxation had been determined by a fall on the hand, while the arm was held away from the body, extended and directed forward. The patient dated this fall at six weeks, but she added that the arm during that time had not always presented the appearances it then showed. She asserted that during this period she could put it in and out as she pleased. She then had some difficulty in moving the limb. The arm went out of place sometimes when she endeavoured to extend it much, but she quickly reduced it again in the manner we describe. Two or three days, however, before her admission, a luxation occurred, which she in vain attempted to remedy. Little faith was at first given to this story. Nevertheless the symptoms of luxation *downwards* were manifest, and its reduction by the ordinary methods was attempted. Extension and counter-extension having been suitably performed, from the particular sound which followed, and the change of form in the shoulder, it was thought that the bone had been replaced in its natural cavity. The patient moreover stated, that it had resumed the position in which she was wont to replace it, and in which she could execute her usual movements, though with a certain degree of embarrassment. On examining the shoulder, however, with minute attention, it was manifest that several symptoms of luxation still remained, such as the flattened deltoid and projecting acromion. Extension and counter-extension being accordingly resumed, after some trifling exertion the bone slided really into the glenoid cavity, the shoulder regained its natural conformation, and every symptom of dislocation vanished.

The circumstances in this case, however, admit of a sufficiently easy explanation. The head of the humerus had been luxated downwards; after having glided from above downwards on the articular cavity, distended and ruptured the fibrous capsule, it placed itself on the inner edge of the anterior or axillary border of the scapula, and a little in the subscapular fossa. In this situation the patient executed some motions of the arm, until she brought the head of the humerus on the scapula, immediately below the glenoid cavity. There making it lean for support, she could use the arm, though with some difficulty, as if it had not been dislocated. The efforts which she made to establish the relations of the articular surfaces could, however, only end in giving the bone a position from whence it should inevitably be dislodged by any violent or extended motions. Accordingly, more decided symptoms of luxation soon supervened.

Treatment of Luxations of the Humerus.

We shall conclude this lecture by some remarks on the *treatment* of luxations of the humerus. This, as in luxations, generally may be divided into three kinds; the preparatory, the reductive, and the consecutive. Though, usually, simple dislocations of the arm may be complicated with œdematous swellings of the upper extremity, with paralysis, with a wound of the axillary artery, with emphysema, or inflammation of the articulation. All these accidents become the objects of special indications. The œdema is rarely seen but in the luxation *downwards*, and is occasioned by the pressure of the head of the bone on the veins and lymphatic vessels. It disappears after the reduction, or yields quickly after it, to the application of a roller, imbued with some resolvent fluid. Inflammation, according to its intensity, must be combated by local or general bleedings, by baths, and emollient applications and fomentations. Partial or general paralysis of the muscles of the upper extremity, a symptom frequently observed, occurs, when either the circumflex nerve alone, or, what is more common, when all the nerves of the brachial plexus, are distended or contused by the head of the humerus at the moment of its dislocation. Antiphlogistics, rubefacients, blisters, or moxa above the clavicle, form the items of treatment in this affection, when the nerves have been only compressed, and in this case the complication admits of a cure; but when the nerves have been disorganised, the affection is hopeless.

The lesion of the axillary artery is very rare, and happens more usually during reduction than at the time of the luxation itself.

In order to facilitate the reduction of old dislocations, baths, cataplasms, emollients, rendered narcotic by the addition of laudanum, extract of aconite, hyosciamus or belladonna, are advisable, and in young and vigorous patients bleeding should be resorted to.

To reduce the luxations in the ordinary manner, the patient is placed on a chair. The middle of a loop formed by a folded napkin is placed on the back of the hand, and its ends turned over to the palmar surface and twisted. This extending noose is

fixed by a circular bandage: a sufficiently voluminous fold of linen is then placed in the axilla, in order to prevent the undue action of the counter-extending noose on the great pectoral and trapezius muscles. The counter-extending loop is made of a cloth folded like a cravat; its middle part being applied on the fold of linen, its ends are brought, one forward in front of the chest, the other behind, and both crossed over the sound shoulder, fixed to a ring in the wall, and confided to an assistant. A number of assistants, proportioned to the necessary efforts, seize the extending noose, and await the gesture of the surgeon to commence. If the luxation be *downward*, the extension is at first in the same direction as the displacement, the arm is then brought downwards and forwards, while the surgeon leaning the external side of the elbow against his chest, brings the head of the bone *upwards* and *outwards*. If the luxation be *inwards*, extension is made *outwards* and *backwards*; and when the head of the bone is disengaged, the surgeon moves it *outwards*. If the luxation is into the subspinous fossa, the extension should first be from *behind forward*. According as the head is freed, the operator pushes it in the same direction, and then the extension is made more directly outwards.

Sometimes, especially in old dislocations, there are difficulties to be overcome which depend chiefly on the resistance of the muscles. To conquer this, various violent machinery, as dangerous as useless, was formerly employed. For this, the modern surgeon substitutes the *diversion of the patient's attention*; a moral method, which, in a practice of twenty-five years, has failed no more than once every second or third year.

The noise of the returning head of the bone, the re-established form of the joint, and the facility of motion, indicate the replacement of the bone. The reduction finished, the limb is maintained in a proper position against the trunk by a bandage which passes round the body, and by a sling which supports the elbow.

The accidents which sometimes complicate dislocation may also be the effect of reduction, and especially in those cases of long duration. Nevertheless they are infinitely less frequent than some persons pretend, as is proved by the history of the numerous series of old luxations which we have given in the tabular form. The chance which, in the short space of three or four years, supplied M. Flaubert with a collection of all the accidents which reduction can occasion is doubtless extraordinary, and the cause of it is perhaps to be sought for in particulars, with which we are unacquainted. The emphysema of the chest, which this surgeon witnessed in one of his cases, was seen also by Desault. In similar cases it is advisable to cover the tumour with resolvent applications, and subject it to graduated compression by a bandage, which would thus fulfil the double object of securing also the arm to the trunk.

Respecting the old dislocations, in conclusion, it will be useful to observe, that, according to the facts we have related, their reduction may be obtained without running the risks which so many apprehend. If we proceed according to known facts, M. Sanson's case shows the practicability of reduction within 96 days. But it is equally evident that the term for the operation cannot be indefinitely prolonged.

On the Influence of Physical Agents on Life. By W. F. Edwards, M.D., F.R.S., &c. Translated from the French. By Dr. Hodgkin and Dr. Fisher, with an Appendix and Notes.

(*Continued from page* 334.)

We resume our examination of this valuable work.

The fourth part, which treats of the influence of physical agents on man and vertebral animals, will form the subject of our analysis in this number. It consists of seventeen chapters, the first of which is devoted to the consideration of "the modifications of heat in man, from birth to adult age." Dr. Edwards here successfully establishes the analogy between man, and the other warm-blooded animals previously examined. In all, the power of *generating* heat was at the minimum in infancy, and increased with adolescence. Thus, a healthy seven months child evolved heat equivalent to 89.6° Fahr. In ten healthy infants, from a few hours to two days old, the mean was 94·55. In twenty adults, the mean was 97°. The second chapter is entitled "On the influence of cold on mortality, at different periods of life." Experiments on kittens and puppies are adduced, which prove that these young animals are capable of enduring, with impunity, a degree of cold which would prove injurious or fatal at a more advanced period of life. The reduction of bodily warmth is therefore less pernicious in its permanent effects, in proportion to the youth of the animal, and, further, the curious relation is

to be noticed, that as the power of developing heat increases, that of supporting a reduction of temperature is impaired. Reviewing then, generally, the facts relative to the influence of cold at different periods of life, we find that there are two things to be distinguished—the cooling of the body, and the temperature capable of producing this coolness. The first of these is, abstractedly, less injurious in proportion to the youth of the animal. But, on the other hand, though young animals suffer less from the same degree of temperature, they nevertheless cool more rapidly than adults of the same species.

The "momentary application of cold" is considered in the next chapter. It is here shown, that although the body may regain its original temperature after such exposure, still that the faculty of resisting future decrements of heat is much diminished. Precisely the counterpart of this takes place with respect to the momentary application of heat, the effect of which is, within certain limits, to enable the body to resist cold with greater vigour. When, however, we follow Dr. Edwards into his fifth chapter, and trace with him the "influence of the seasons on the production of heat," we find, that, in this respect, warm-blooded animals may, with propriety, be divided into two classes; first, those whose constitutions harmonize with the climate; and secondly, those whose constitutions are not adapted to it. With the former, as the external temperature falls, their internal power of generating heat is augmented. The latter, on the other hand, possess not the same resources, and with them an opposite effect is produced. The hibernating mammalia afford the best example of these individuals, and traces of the operation of the same law are not unfrequently to be detected in other warm-blooded animals, and in man.

The detailed analysis we have already given of the author's experiments on the Batrachian reptiles and fishes, precludes the necessity for our following him with equal minuteness through the five subsequent chapters. In these he treats—Of the analogies between the phenomena of asphyxia in these animals and man—Of the modifications of respiration dependent upon species, age, &c.—Of the continued action of air and temperature—Of the effects of temperature upon the functions of circulation and respiration—Of the influence of the respiratory movements on the production of heat; and, lastly, Of perspiration. The chapter on perspiration is subdivided into several sections, in which are separately considered the influence of meals, of sleep—of the hygrometric state of the air—of the motion or rest of the air—of the influence of the atmospheric pressure, and of temperature. In this chapter are also examined the phenomena of perspiration by evaporation and transudation — cutaneous and pulmonary perspiration—and, lastly, perspiration in water. The striking analogies observed between the results of the author's experiments on these points and those already detailed, preclude, as we have observed, the strict necessity of our making these chapters the subject of special analysis.

In the twelfth and thirteenth chapters, Dr. Edwards advocates the affirmative on the controversy relative to absorption in water and humid air. In the fourteenth, the author presents an excellent coup-d'œil of the experiments of other writers on the degree of heat man can endure, the influence of heat on the temperature of the body, and on refrigeration in air at rest, and in motion. From these experiments Dr. Edwards derives his own conclusions, which generally coincide with those of the experimentalists themselves.

We now arrive at the three last chapters, in which the full genius of the author is displayed, and in which we find the details of those celebrated experiments the fame of which has rung through the scientific world. That on the influence of light upon the development of the body, shall first engage our attention.

The influence of this agent on vegetables is sufficiently established by the phenomena of blanching or etiolation. In the mineral kingdom, again, its power is manifested in numerous syntheses and equally evident decompositions. In man, even, as a late author-tourist has correctly remarked, the positive agency of the sun's light in the sustenance of health, may be inferred from the palpable etiolation effected by the midnight vigils of fashionable life. Dr. Edwards, however, seeing the many fallacies with which observations on man are liable to be encompassed, attempted to investi-

gate the subject in the lower animals. The first comparative experiment was on the spawn of frogs, two portions of which were exposed to the necessary warmths, one in the light, the other in darkness. The latter were not vivified, while the former were developed in succession.

Again, two tadpoles were placed in large vessels containing each ten litres of water, into which the light did not penetrate; others were placed in transparent vessels; whilst all the latter attained their second, or reptile form, one of the others retained that of the tadpole, although it had doubled in weight. The presence of solar light, therefore, favours the development of organization or type, although its absence does not oppose increase in size. A singular fact in natural history further illustrates this law. In the subterranean and dark rivers of Carniola, the *proteus anguiformis* passes its life in the imperfect form which, in waters illuminated by the solar rays, is peculiar only to the second stage of its existence. A further example of the operation of light upon form may perhaps be found in the general symmetry of shape in those climates where nakedness is not prejudicial to health. Many facts in the history of scrofula seem also to be connected with this principle.

The next chapter is perhaps the most important in the entire work. In it the alterations of the air from respiration are examined. In the first section it is shown that the proportions of oxygen which disappear, and of carbonic acid produced, vary according to the *species* of animal employed in the experiment—a most interesting fact, and one which, at the same time, establishes a valuable principle in the regulation of our theoretical views on the function of respiration, and satisfactorily explains and reconciles the different results arrived at by other trustworthy experimentalists. The second section relates to the proportion of azote in the inspired and expired air. On this subject previous chemists had arrived at the most discordant conclusions, some maintaining that the azote expired was equal to that inspired; some, on the other hand, holding it to be greater, and others less. Numerous experiments were consequently instituted by Dr. Edwards on guinea pigs and birds, and during the spring and summer it was unequivocally ascertained that the exhaled azote predominated. But in the experiments made in autumn and winter, *the reverse was equally proved.*

The production of carbonic acid in respiration, is next investigated. To account for this there are two principal suppositions; one that the carbonic acid is formed directly in the lungs; the other, that the carbonic acid already formed in the blood, is exchanged for oxygen in those organs. To decide between these conflicting opinions there is only one mode. It is evident that if the carbonic acid be formed by the carbon of the blood and oxygen of the air in respiration, it will not be produced in the case of an animal respiring a gas containing no oxygen. A frog was therefore taken, and its lungs were compressed to expel the contained air. It was then introduced into a vase of perfectly pure hydrogen gas. It lived for more than eight hours, and the air on analysis yielded a quantity of carbonic acid nearly equal in bulk to the animal itself. The acid was therefore entirely the product of exhalation. The experiment was extended to other cold-blooded animals with the same results. A kitten was then taken and similarly treated. In the residual air a quantity of carbonic acid was found nearly double what could be formed by the entire oxygen of the quantity of air the animal's lungs were large enough to contain. Unquestionable proof was thus obtained, that at least a part of the carbonic acid of respiration is formed independently of the immediate combination of oxygen and carbon during inspiration. But though a part is thus formed, is the whole equally so? May not a part be also formed directly in the lungs? The negative of this question Dr. Edwards has distinctly established, by showing that frogs evolve, under favourable circumstances, as much carbonic acid in hydrogen as in atmospheric air. What then is the *source* of this acid? The experiments of Vauquelin on the blood suffice to demonstrate its existence in the mass of the circulation.

The last chapter of the work consists of applications of the preceding facts to various physiological and pathological phe-

nomena, and to the improvement of hygeienic precautions.

First, with respect to the power of producing heat. This varies with the individual, with his state of health, and disease. Reduced power usually shows itself in two forms; torpor, and increased rapidity in the movements of respiration and circulation. No organization seems absolutely to be incompatible with the former. In individuals thus constituted, the state of sleep approaches to that of hibernation, the power of generating heat is diminished, and the application of cold, which would be harmless when awake, becomes most pernicious to the sleeper. The phenomena of *trance*, or lethargic sleep, further coincide with the inferences Dr. Edwards would establish. In the second variety, again, of deficient generation of heat, increased circulation and respiration are occasioned. In man this condition bears strong affinity to the early stages of intermittent fevers. In these, according to the injury originally done to the power of producing heat, the cold stage will be of proportionate duration, —from the mild degree in which health is re-established after a single attack to that in which death takes place in the second or third accession. The beneficial influence of external warmth in the alleviation of these maladies, is further an illustration of the application of our author's experiments. M. Chomel has thus found that the vapour-bath is of signal utility in the treatment of intermittents, while the warm water-bath is of little service. The explanation is obvious. A higher temperature can be borne in the former medium; and, moreover, the vivifying influence of the air on the skin is also preserved.

We pass over the paragraphs relative to the influences of climates, and the phenomena of the production of cold by exposure to dry and moist air, especially since considerable obscurity still overhangs the latter branch of the subject.

In the sixth paragraph the author shows that *evaporation* is not the *sole* cause which, under exposure to heated atmospheres, keeps the temperature of the human body below that of the surrounding medium. In the seventh it is proved, that the temperature of warm-blooded animals varies with the season of the year, within certain and moderate limits. A fact is adduced to show, that, in disease, the human body may attain the temperature of 110.75° Fahrenheit, being 12½ degrees above the natural standard. In the repression of morbid heat, cold douches, affusions, &c., act doubly, viz. by the immediate cold produced by their evaporation, and by the effect of this cold in preventing the *future* generation of heat.

With respect to the occasional evil effects of perspiration, the twelfth paragraph contains several ingenious and useful observations. The author here distinguishes between cutaneous and pulmonary perspiration. Considerable evaporation from the surface of the lungs may take place, independently of any from the body. In ascending high mountains this frequently occurs, and the distress thus occasioned is vulgarly referred to the rarefaction of the air. Similar phenomena occur during frosty weather in persons occupying rooms heated by stoves. Popular instinct, indeed, has already detected the true cause of the distress thus occasioned, and we accordingly find a basin of water frequently placed on the stove.

The remaining paragraphs of this chapter do not require especial notice, inasmuch as they chiefly relate to the blood, and contain no facts with which the readers of THE LANCET are not familiar.

We have thus completed our examination of this important contribution to British medical literature. Dr. Hodgkin deserves the gratitude of his professional brethren for the pains he has taken in supplying them with such valuable materials for the improvement of their present knowledge, and such a model for their imitation in the prosecution of new researches. In Dr. Edwards, thirst for discovery is not synonimous with greediness of fame, and invincible tendency to premature generalisation. Every line of his work bears the stamp of unremitting labour, strict impartiality, patient and sagacious induction, and, what is most precious of all, indisputable, self-evident *truth*,—a quality which we conscientiously believe to be more rare in the writings of modern experimentalists, than is generally suspected by an over-confiding profession. So general, indeed, has become the infamous vice of describing experiments never performed, and thence drawing, with

unblushing effrontery, inferences of the highest importance to science and humanity, that, for the future, before confidence can be placed in the statements of any experimentalist, he must be prepared with numerous trust worthy witnesses of his labours. The Lancet, at least, shall act upon this principle, and having given this notice of our intentions, future authors must not be indignant if we rigorously scrutinize even the character of the evidence they may adduce in authentication of their statements. Competent anatomists will thus be required to testify to anatomical discoveries; practised and distinguished chemists must be the witnesses in the chemical department. The public will not thus be made the victims of those specious manœuvres, which we have of late so frequently seen practised by pretenders to discovery in the several departments of science.

A copious appendix closes Dr. Edwards's work. It contains some papers of great value, and others of comparatively trifling importance. In the first class we may enumerate, 1. A chapter on electricity, translated from the French of MM. Prevost and Dumas; 2. On muscular contractions produced by bringing a solid body into contact with a nerve without a galvanic circuit; 3. On atmospheric electricity, by M. Pouillet; 4. Observations on the same subject by Messrs. Howard and Hodgkin; and 5. Microscopic observations on the blood and animal tissues, by Messrs. Hodgkin and Lister. We except from this commendatory catalogue the reprint of the thesis on absorption, defended by Dr. Hodgkin in Edinburgh; not from any absolute deficiency of value in the productions, but from the bad taste displayed in the resurrection of so much pompous Latin from the grave of obscurity into which it had happily sunk. The profession have long since learned to estimate at their true value these caricatures of the classics. How extremely ill-judged is the re-parading of Dr. Hodgkin's *three* dedications to the aforesaid thesis. In number 1, he invokes Humboldt with an "*O! Utinam pro ipsius mentis dignitate possem referre.* In number 2, Dr. Duncan is thanked, "*Ob paternam solicitudinem quâ me dum scarlatina laborabam curavit.*" In number 3, again, Dr. Hodgkin, "in Æsculapii ædem ingrediens," addresses his orizons for his father, "*ut diu vivat* PANCRATICE *valeat*, &c. This is, no doubt, all very amiable, but sensible men must laugh at it, and especially in a second edition, and it has, moreover, the disadvantage of attracting ridicule to an essay which in simple and unaffected English would have excited considerable attention.

Despite the plague of anglicising it, however, and our mortification at being caused so much needless trouble, we will wade through the swollen tide, and present our readers with a notice of the good things it contains in an early Number, when we shall also analyse the several papers of the Appendix, which we have favourably noticed.

A Brief Outline of the History and Progress of Cholera at Hull, with some Remarks on the Pathology and Treatment of the Disease. By James Alderson, M.D. London: Longman and Co., 1832. 8vo. pp. 58.

We notice this little work chiefly because it contains two well-executed coloured plates, exhibiting the appearance of the intestines in cholera. This has been the first attempt, as far as we remember, to transfer to paper the internal pathology of this formidable disease. The volume itself contains nothing which is remarkable or worthy of notice, being, indeed, rather a few detached observations and reflections than a systematic account. One doctrine alone is conspicuous for its novelty, and, if we mistake not, its complete fallacy. Dr. Alderson regards the fluid poured out by the intestines as an inflammatory product, and exactly analogous to the exhalation which takes place from the serous cavities under the excitement of inflammation. We would beg to remind Dr. Alderson, that serum is not the inflammatory product of a mucous membrane, though it may be of a serous; and, further, request him to consider and compare the morbid effusions of gonorrhœa, catarrh, dysentery, &c., with that which results from cholera. The former are examples of true inflammatory products, the latter is a case more analogous to venous hemorrhage than to any other action.

THE LANCET.

London, Saturday, December 15, 1832.

If the lecturers of the hospitals of London be accessible to a feeling of shame, surely their dismal, yet hardened, faces, will exhibit some evidence of remorse, when they peruse the invaluable lectures of Baron DUPUYTREN, reported in this Journal—lectures to which we must again direct the especial attention of the reader. Be it remembered that the doors of the Hôtel Dieu are thrown open to strangers as well as to natives, and that the Baron, disdaining to extort money for delivering discourses which may be extracted from any Dictionary of Surgery, or any sustained composition on that branch of the science of medicine, lays open the book of nature to the delighted view of the beholders, and founds his rational deductions upon the facts which the student has *himself* the opportunity of observing. Contrast this generous, this noble treatment of the untaxed inquirer in the Hôtel Dieu, with the discipline to which the pupils of the London Hospitals are called on to submit. First, by the regulations of the colleges and company, he is required to pay for certificates of attendance on the medical practice of various charitable institutions, and the lectures delivered therein. Secondly, when he attends to witness that practice, he is permitted merely to walk through the wards, and take a distant view of the sick, without being furnished with any explanation of the characters of disease, or the *rationale* of remedies; and, Thirdly, on entering the lecture-rooms, his mind is stultified by hearing a dull reading of still duller essays, which, were he so disposed, he might peruse at home at his own fireside. Thus, having lost his money, he is called upon to make a sacrifice of his time; and, finally, from the innumerable defects of the system of instruction, experiences no small difficulty in undergoing the ordeal of a superficial examination, before the boards of medical scrutators. The neglect of clinical instruction, by our hospital physicians and surgeons, has become a disgrace to the enlightened age in which we live, and, independently of the imputation which it casts upon those individuals, of a want of medical knowledge, it tends to show, in no small degree, that these monopolists labour under a moral infliction, which is equally striking and debasing.

Again, then, do we call on the students of London to examine for themselves the clinical lectures of Baron DUPUYTREN, and, having done so, let them reflect on the consequences to themselves of suffering nearly the total loss of that information which the mass of accumulated diseases in our hospitals is so efficiently, so richly, capable of furnishing. It is scandalous that a lecturer in a public institution should be permitted to pocket between two and three thousand pounds per annum, for drawling out, session after session, the same identical set of unintellectual rigmarole discourses,—as though no new facts had been observed, and as though the *principles* which should guide medical practice cannot be found in an eight-shilling octavo. This fraud, so long committed on the pockets of the students, and on the patience of the community, must and shall be abolished.

Having in no one instance yielded an hypocritical compliance with the cuckoo cries of ignorance and prejudice, we are enabled on the question of the ANATOMY ACT, as on all other subjects, to speak out boldly, both to the profession and the public. Dissection of the dead must, as we have always contended, be continually submitted to for the security of the living. If there be no knowledge of anatomy, medi-

cine cannot be cultivated as a science, and the practice of that branch of knowledge would be thrown wholly into the hands of empirics. Need we say that thousands of the community would annually fall victims to such a barbarian system of JOHN-LONG CRAFT?

The wild and irregular public discussions concerning the misappropriation of dead bodies, have, as we predicted, already had their pernicious effect in more quarters than one, and, without being calculated to give any relief to the parties who were most aggrieved, are likely to deprive those students who were more fortunately circumstanced than the complainants, of the means of acquiring a knowledge of their profession. We deeply commiserate the present lamentable position of the teachers and the pupils of the private schools; but the power to relieve themselves from the dilemma in which they are placed, will not be strengthened by producing a feeling of dissatisfaction in the minds of those of their contemporaries who are not altogether dissatisfied with the existing arrangements. In a word, there must be a sympathetic acknowledgment of wrong doing, and a concurrent demand for redress. The voices of *all* the schools should be raised in one harmonious prayer to the legislature, for an amendment of the ANATOMY ACT.

The buying and selling of the dead must be WHOLLY INTERDICTED, and bodies for dissection should be decently and equitably distributed under such regulations as were explained in the pages of the last LANCET. This, however, must always be borne in mind, that any improvement in the present system, must be founded on a total abolition of the immoral, indelicate, and disgusting practice, of trafficking in the remains of deceased human beings—a species of trade which cannot be tolerated in any country, where cannibalism is unrecognised as a part of the *social* system of its inhabitants.

WE record with feelings of more than ordinary regret the death of the celebrated Dr. SPURZHEIM. From an account in an American newspaper, we learn that he died of fever at Boston in October last.

SPURZHEIM was, for a long time, the coadjutor of GALL, but mutual jealousy, it was alleged, led to their separation at a time when the reputation of these physiologists was rapidly spreading throughout Europe. GALL was a genius. SPURZHEIM was not, though his concentrativeness and power of applying to study gave him many advantages over numbers to whom the title of originality strictly belongs. SPURZHEIM, though not celebrated as a discoverer, could handle with singular facility the materials which were furnished by men of a more highly gifted class. Thus, after GALL had laid out the groundwork of the science of phrenology, SPURZHEIM entered the field, laboured assiduously, and gave form and consistency to much matter which had been left in a crude state by the enterprising discoverer.

SPURZHEIM was the author of several works (some of them in conjunction with GALL) on the "Anatomy and Physiology of the Brain;" an excellent treatise on "Insanity," and, lastly, a small treatise on "Education," which ought to be in the library of every parent and guardian, and every instructor of the youthful mind. Dr. SPURZHEIM was a man of modest, unpresuming demeanour, and instructive in conversation, but almost exclusively devoted his time to the study of the science of phrenology. His lectures were attended by brilliant audiences, not only in this metropolis, but throughout the whole of England, and his labours contributed in a higher degree than those of any other man to make known to the scientific world the actual physiology of the human brain. Dr. SPURZHEIM was upwards of fifty years of age, a widower, and has left no family.

Cholera.—"A word which has headed so many articles in the English and foreign medical journals during the last twelve months, that its mere exhibition in title-page type, has become in the eyes of professional men what the sight of a gun is to a crow. Let the field promise him never so full a meal, he will turn from it sharply enough."

Such is the summary explanation which, had the early numbers of Messrs. Sherwood's *Cyclopædia of Medicine* yet to be written, might, not untruly, be attached to the awful word which stands first above. So far true indeed is the explanation, that medical journalists may judiciously be loath for some time to come (were there no other reason) to make the term again a prominent word in their publications.—Not because there is nothing left uncommunicated to the public on the subject, which it is desirable for them to know, but lest valuable information should, from the satiety that has been produced, be passed heedlessly over in a desire for other food, which, though not more nutritious, may be less palling to the taste. Partaking of this fear, we have lately withheld for a time from our pages many communications received from the Central Board of Health, and various able correspondents. This restraint of course must only be temporary. The subject is infinitely too important, not only to the people of this country, but to the entire human race, to be lost sight of in the present imperfect state of our knowledge relating thereto. We have no desire, however, to write a chapter on the topic just now, but our attention being called to some points connected with it, during the last week, we devote a brief space of this portion of our journal to two cholera questions put to us by correspondents, one of whom inquires whether we have read the preface to Dr. Hamett's lately published "History of the Dantzic Epidemic,"* and asks us, in addition, "whether we ever knew a man so ill-used."

To the former question we answer, "Yes;" and shall probably serve the chief purpose of our querist, by recommending our readers also to peruse it. We do so, first, because as Dr. Hamett thinks he has, and appears to have, suffered wrong, we would be among the first to endeavour to obtain for him that partial remedy for an injury which general notoriety of the facts often obtains in public matters; and secondly, because, while perusing the polemical statements of the author, the reader will also have in his hands the scientific contributions of the doctor to the history of the cholera. Whatever views may be founded on them, whether in accordance with or against those of the author, his statements demand a perusal. At the same time, it is our duty to inform our readers that they will have to pay the sum of ten shillings for the book; which is rather large, by comparison, as regards quantity, though it may not be so as to quality. This recommendation is to be received as no approval of any doctrine supported by Dr. Hamett. We are at issue with him, wholly, in some of his conclusions.

With regard to the author's "ill-usage," we shall briefly detail the following particulars. Dr. Hamett was sent by Government, in June 1831, to Dantzic, to investigate "the epidemic raging in that city." He accomplished the object of his mission in a very diligent and praiseworthy manner. When his reports were received in London, a committee of the College of Physicians were requested, by the Government, to make a digest of the facts contained therein; but the M.D.s being, probably, rather a lazy set of gentlemen, had not performed their task by the time of Dr. Hamett's own arrival in town, and they therefore transferred the duty to the author himself; to perform which he received back all the reports but one; which report Dr. H. regards as a most important one, on the ground that it contained "a mass of circumstantial evidence in support of his conclusions that the disease had not been imported into Dantzic, and that it did not prove contagious in that city." (p. viii.) For this document he made "proper inquiry;" but it certainly does not appear in

* Substance of the Official Reports upon the epidemic called Cholera, which prevailed among the poor at Dantzic, between May and September, 1831, &c. By John Hamett, M.D. Highley, 1832. 8vo. pp. 129.

Dr. HAMETT's statement, so clearly as it ought, how and why it came to be deficient. There is an hiatus here, which a little more boldness on the part of the author might have supplied, and which, to secure full sympathy on the part of the public, Dr. HAMETT should have filled with facts, and names in full detail.

He says, that the Board of Health then existing,—a Board, which, by the by, it will be remembered, THE LANCET entirely demolished in a couple of weeks,—certainly received (for it acknowledged the fact) the report in question; but that one of the members, who did not hear it read publicly (diligent member!), "took it home, and never returned it," and the secretary was "unable to recover it." And who, pray, was the borrower—the purloiner? What did he say to the secretary, when that gentleman sent to him for the document? Or if Dr. HAMETT knows the name and the motive of the implied thief, why does he not announce them? and, if he be not in possession of these particulars, why is he not acquainted with them? The legs of Dr. HAMETT are not tied, nor are the lips of the official sealed, that the one cannot go to learn, and the other cannot tell, these essential portions in the history of the wrong complained of. The truth, however, should be extorted, from some party or other, by THE LANCET, were it not that the public itself proves to have suffered no injury from the detention; for Dr. HAMETT had, it appears, got rough drafts of all his reports properly authenticated, and the stolen paper is now in print. With regard to the motive for concealment, the following is given as a key to it:—

"Finding, on my arrival, that the principle of contagion had been adopted by Government, and as my details upon the subject, with the inferences drawn from them, were decidedly in opposition to that principle, I had some reason to apprehend that such extracts as I had to make from my official reports, would not be published. Still, as containing so many facts of high importance to the public, I imagined they would not be overlooked."

But there is another charge against some "power behind the board," or *in* the Board, and in this case the party wielding the wand of suppression is proclaimed, though his sin is, perhaps, of a rather more venial shade. The Lords of the Privy Council ordered the paper drawn up by Dr. Hamett from his reports, to be printed. Upon this, first of all, the Superintendent-General of Quarantine attempted (says the Doctor) to induce him not to print the extracts thus authorized, and then when a proof copy was sent to the said S. G. Q., as the property of the state we suppose, the work was not published, but an intimation was conveyed to Dr. H. by the Superintendent "that the Privy Council did not sanction its publication," which, says the Doctor, "from what I have just stated, and according to what I have since learned from authority, could not have been the case." An "ultimate reason," however, was given for the "delay," as the author terms it, through "the publisher of the official reports upon cholera to Government," and that was, that it was intended to bind up and circulate them with the second edition of the St. Petersburg reports, not completed. Dr. Hamett evidently does not believe a word of this excuse.

With a just sense of the profitless labour which a detail of further items would require from him, Dr. Hamett dismisses the subject of his complaints in the following sentence. "It would be out of place to enter into a detail of the trying difficulties and vexations I met with for some time (relating to this matter) after my arrival in England. I have the clearest evidence that the character of my circumstantial and complicated labours was misrepresented. This I could not have anticipated." We sincerely commiserate Dr. Hamett on his having suffered annoyances which are so destructive of the quiet that is essential to scientific men when they are in the just and steady execution of important vocations. Before laying aside his volume, however, for the present (for of all that is valuable in it we shall not lose sight when placing on record the result of our historical investigations into the disease), we must adjudge Dr. Hamett a stripe for putting on paper the following sentence:—

"For my own part I feel persuaded, that the *impartial* and *most experienced*, as well as the *scientific* and *talented* of the profession, will have no hesitation in admitting, from the multiplicity of facts which I have

adduced, that *I am right* in my conclusions on the essential point, that cholera is *not* contagious, or communicable, as it is now called, in its nature."

A very modest and complimentary declaration indeed! Why, there is enough in this one sweeping avowal to convince every really scientific man in the profession, that the philosopher who could make it has but half a claim to that name, and is the very man whose conclusions are *not* likely to be right in a great question requiring the best powers of reasoning and deduction, and the calmest and most unprejudiced state of feeling that ever was brought to the consideration of any question yet proposed since Bacon wrote the unscientific, inappropriate, and injudiciously-quoted passage, which is prefixed as a motto to the title-page of Dr. HAMETT's work. A word more. Dr. HAMETT puts the words "*now called*," in the above sentence, in italics; and he does so with a view to make the type spare him the trouble of writing an argument thereon. Even the contagionists, he would say, have changed their opinions, and *now* use such and such language. This is thoughtless enough when made the basis of an argument in the same volume which contains the statement of his coadjutor and supporter, Dr. BAUM (page 71), that "there were but five patients who *caught* the disease in his hospital." There is every day a great deal of writing without thinking, and a vast deal of thinking not worth a letter of writing. *Ex. gr.*

We turn now to the letter of the second correspondent to whom we have alluded. This gentleman sends us an argument, and a bit of news relative to the cholera, in rather a triumphant spirit,—he being a non-contagionist. He says that in a struggle for a vacant membership of the Academy of Medicine in Paris, a celebrated non-contagionist, M. CHERVIN, the coryphæus of that party in France, has just been elected to the honourable post in the teeth of M. ANDRAL (*fils*), who holds a contrary opinion, by a considerable majority of votes. From this fact, he would impose on us that our scientific medical brethren in that institution, to whose judgment we ought to bow, almost universally entertain views adverse to the doctrine THE LANCET has maintained with respect to irruptions of the cholera.

Our correspondent may be assured that his inference is totally erroneous. The election was altogether a political one. The fact is, that, with the exception of the household physicians, the *juste-milieu* government of France has scarcely a friend in the Academy. M. ANDRAL is the son-in-law of M. ROYER COLLARD, a *royal* physician, the HENRY HALFORD (almost) of the court retainers, and a thorough-bred *doctrinaire*. A *ministerial* canvass was consequently commenced for M. ANDRAL, when M. CHERVIN adroitly availed himself of the political feeling which reigned in the *Académie*, seized the opportunity which was thus open to him, offered himself (an excellent "radical") in opposition to ANDRAL, and was triumphantly elected. The question of professional views was, in this instance, allowed to stand entirely aside; and, in truth, any other talented candidate would have been equally successful in the contest with the protégé of the detested ministry.

WESTMINSTER MEDICAL SOCIETY.

Saturday, December 8th.

MR. PETTIGREW *in the Chair.*

INSTRUMENTS FOR BREAKING CALCULI IN THE BLADDER.

AFTER the minutes of the last meeting had been read, the PRESIDENT observed, that as Baron Heurteloup had been invited to attend the meeting this evening, for the purpose of showing and explaining the nature of his instruments for breaking stones in the bladder, and was now present, it was desirable to waive the reading of the paper promised on phrenology, and hear at once the statements of that gentleman, which were communicated in the form of a memoir in English, read by Mr. Burnett. The memoir commenced with some general observations on the operation of lithotrity, and on the impropriety of persons unaccustomed to its performance on the living subject venturing to condemn instruments that had been successfully used, and proposing as improvements, mere alterations, often to the injury of the instrument. The Baron then detailed the circumstances which had suggested the construction of his *percuteur* and led successively to its present form. These were, first, a case occurring six years

ago, in the Hotel Dieu, in which, in consequence of the pulley of the drill being wanting, the rod of the instrument was used as a percussor, the calculus broken, and much detritus voided. The second was a case operated on in Greenwich Hospital, in which the shell of a calculus, previously excavated, was broken down in a somewhat similar manner, means being taken to prevent the piece moving and the claws opening, so that a hammer could be used to increase the force of the percussion. Two years ago a third attempt of the same kind was successfully made, showing, however, the necessity of further improvement in the apparatus. This led the Baron to construct numerous experimental instruments, the models of 70 or 80 of which were exhibited. The principle was the same in each, viz. that of forming two planes, rising perpendicularly from the axis of the sound-shaped percuteur, and moving one against the other, so that the calculus might be passed and crushed by the force of a hammer, externally applied, the difference consisting in the greater degree of strength given to the last-formed instruments where they are of reduced size to suit diminished urinary canals. The instruments now constructed are proved, before use, to be able to crush, without a strain, calculi harder than any that are met with in the bladder, and able to resist a much greater force than is ever employed in actual practice. The memoir likewise dwelt on the necessity of having the fixed point formed by the rectangular bed in this operation; and concluded by inviting as many members of the Society as the author's room would admit, to witness an operation on the living subject on the following Monday. Baron Heurteloup then demonstrated the manner in which the calculus was grasped in the bladder, the way in which the instrument was fixed (having his rectangular bed present), and how the stone was comminuted.

When the Baron had concluded, Mr. Costello begged to be allowed to examine his last improved instrument, but this the Baron refused.

Mr. Costello then rose and said, it would be too long an occupation for him to follow all the points in the memoir. An instrument had now been exhibited which was said to be the *ne plus ultra* of perfection; at least it was the last the Baron had made. He (Mr. C.) had desired to see it, the Baron had refused the request, and it was now only necessary to appeal to the number of instruments on the table to be persuaded that defects had existed. When he (Mr. C.) wrote to the Academy of Sciences in Paris, and reasoned on those defects, his reasoning was conclusive. A fortnight since he pointed out a few steps in the progress which had ultimately led him to the construction of an instrument which he considered perfect. It was to be understood that he rested his claims upon the ground of *perfection* in his instrument. He was willing to abide the determination of the committee, which had been asked to deliberate on this question, and he asserted, and was fully prepared to prove, that M. Heurteloup had no instrument so safe, so effectual, and so simple, as that which he (Mr. C.) had exhibited. His instrument, together with M. Heurteloup's, should be placed in the hands of as many eminent and scientific surgeons as might be wished for by either party, and they should have an opportunity of witnessing his operations on the living subject (which were not few), and he would then challenge a comparison.

The Chairman said he was not present at the late meeting, but from what had been related to him, he thought that Mr. Costello must have mistaken the object of the Society in the invitation to Baron Heurteloup to exhibit his instruments. The Society did not wish to make any declaration of opinion as to the original invention of any particular instrument, or even to say which was the best. Minute examinations of the instruments, and the witnessing of a great number of operations on the living subject alone, could enable any man, or body of men, to come to a decision on such points. Any discussion upon the respective merits of the instruments had better take place in the medical journals of the day, through which the opinions of the profession at large could be obtained, forming a far more competent forum than the Westminster Medical Society. Oral observations on the superiority of lithotrity over lithotomy from any gentlemen in the room, or of the best modes of performing those operations, would no doubt be gratifying to the Society, but a personal controversy between the two gentlemen named would be foreign to the business of the evening.

Mr. Hunt considered that Mr. Costello was under no mistake as to a comparison of the instruments being intended. It was originally proposed that a comparison should be made; and a long discussion occurred upon the very word *comparison*. Comparison was the very object of the meeting.

The Chairman hoped to be supported in the views he had taken.

Mr. Burnett objected to the statement that comparison was the object.—The question referred to the *principle* upon which the instruments were made. Mr. Costello's instrument might be better or worse than the Baron's; the point was, whether it was constructed on a new principle, or was only a modification of another instrument. The gentlemen present had seen the instruments,

and probably had, by this time, formed an opinion on that subject.

Mr. Costello denied that the instruments had been seen. He had distinctly asked M. Heurteloup to permit him to examine his instrument, and had been refused.

[Here there was a slight pause, during which Baron Heurteloup opened his case, took his instrument out, and handed it across his shoulder to Mr. Costello.]

Mr. Walker now brought forward the identical instrument which had been made by Mr. Weiss in 1824. The meeting would remember, that a fortnight ago Mr. Costello declared that Baron Heurteloup's percuteur was a copy of this instrument, and not original; and what he (Mr. W.) was now about to state, he was prepared to prove. Mr. Costello, in his letter to the Academy of Sciences in Paris, distinctly stated that the Baron's was not an original instrument, but a copy from Weiss's instrument. If this was disputed, he (Mr. W.) had the letters in his pocket, and would produce them. Moreover, it was stated that Weiss's instrument was the safer of the two.

Mr. Costello. No, nothing of the kind.

Mr. Walker. May I be permitted to read the letters? (Cries of "Read.")

Mr. Chinnock rose to order.

The Chairman here stated, that a third instrument had now been introduced; and if this course was to be pursued, the meeting might be entertained with thirty more. Gentlemen might hunt for dates and authorities; and, by arguing upon comparisons of all the various systems which had been proposed, might turn the meeting into a mere arbitration room.

Mr. Walker said, he had come forward with a mass of information, which he had taken some pains to collect; but, according to the chair, it would appear that he was debarred from laying it before the meeting. At all events, he felt himself at liberty to allude to Mr. Goodwin's case, which had been published. He then commenced giving an account of that case; but, as it has been already in part published in this Journal, and as he was several times interrupted by Mr. Costello and other members, he refused to complete his narrative.

Mr. Costello detailed some particulars of Mr. G.'s case, stating that he had operated upon that gentleman but twice, and each time for a period not exceeding from three to five minutes.

Mr. Goodwin, a visitor for the evening, begged to correct Mr. Costello, and to assure him and the Society that he had submitted to Mr. C.'s operation three times; and hoped Mr. C. would produce the letter he had received from him.*

Mr. Costello pledged himself to produce the letter itself; which, he said, would prove that he had only operated twice.

The Chairman here interfered, and would not allow any further allusion to the subject.

Baron Heurteloup, through his interpreter, now made a short address in further exposition of the nature of his instrument. He claimed the principle of percussion to himself. From the specimens before the meeting, gentlemen would see the immense labour he must have undergone, they being nothing to the number altogether which he had made. He had sent his instrument to the institution in Paris with a view of obtaining the prize, but that had been withheld from him in consequence of the two letters which had been addressed to the Academy by Mr. Costello, and which, he grieved to say, contained statements that were not correct. He could not reply, however, to those statements in France, for they had been sent thither from a foreign country, and all the remedy he had was to make his statements here that they also might be conveyed to Paris from this place. He was desirous that a committee should be appointed to decide upon the relative claims of the two contesting parties, which committee would receive proof that what he advanced was true, and that which had been advanced on the other side was unfounded.

Dr. Somerville lamented that there should have been anything like personalities or acrimonious feeling indulged in on the present occasion, and hoped that this would be the last time they would be witnessed in that Society. He was sure that some of Mr. Walker's expressions must have fallen from that gentleman inadvertently.

Mr. Walker would be sorry to say any thing which he could not support; but if Mr. Costello was permitted to repeat statements as facts, which had been contradicted in The Lancet, any terms of disapprobation he had used, if not justified, were to be excused.

Dr. Sigmond then proposed, and Dr. Copland seconded, a vote of thanks to Baron Heurteloup, which the chairman, on behalf of the Society, presented to Baron Heurteloup in the French language, complimenting him on the "very handsome

* It may be remembered that this case gave rise to some correspondence in the Lancet several months ago. As the discussions was a profitless one to our readers, it is with some reluctance that we again give place to any remarks on the subject. A desire, however, to observe strict impartiality in recording the present proceedings, induces us to waive our objection.

manner in which he had acceded to the request that he would exhibit his instrument," and assuring him of the pleasure which he (the Chairman), in common with the other members of the Society, had derived on perceiving the great degree of perfection to which the apparatus had been brought. The Chairman having, in conclusion, alluded in terms of warm praise on the blessings which had, through the labours of the lithotritists, been conferred to the community, adjourned the meeting until next Saturday.

We publish in this place the following

LETTER FROM MR. WALKER.

To the Editor of THE LANCET.

SIR,—Knowing the value of your pages, and feeling that well-authenticated facts need not lengthened argument for their support, I will be as brief as possible while I endeavour to remove an impression which some of the members of the Westminster Medical Society appear to have entertained, viz. that, in opposing Mr. Costello's claim to originality for an instrument intended for breaking stone in the bladder, I was actuated by improper feelings,—such as I should be sorry to entertain. Mr. Costello presented his instrument to the Society as his own invention; in proof of which I need only refer to the minutes of the proceedings of the meeting of the Society on the 23d November, and to the report of those proceedings in your Journal of Dec. 1st. Many gentlemen present, of whom I was one, having frequently seen Baron Heurteloup's instrument in use, expressed an opinion that the mechanical principle of Mr. Costello's instrument was the same as that of the Baron, which he calls "Percuteur Courbe à Marteau," and therefore it would seem that Mr. Costello acted improperly in not giving just credit to the inventor of the principle which he had adopted and claimed as his own. In reply to which Mr. Costello stated, that he had never seen the Baron's instrument, and therefore it could not be a copy: and Dr. Epps remarked, that as Mr. Costello had never seen it, it was not just of Professor Burnet to call it "something like plagiarism." Mr. Costello, however, in a letter published in The LANCET of July 28th, 1832, in answer to Mr. Briggs, states, "I have seen it; I possess it; I have operated with it, and have modified, improved, and strengthened it."

Mr. Costello also stated, in a letter addressed to the Academy of Sciences of Paris, dated June 15th. 1832, and published in your Journal of July 7th, 1832, in allusion to the percussion instrument, "The curved percussor is not new. Weiss, a cutler of London, made this instrument in 1824. It acts by a screw; Heurteloup's by a process *more violent* and *less safe*, viz. percussion. In Weiss's, pressure is less exerted; if the calculus does not yield, the operator can release it from his grasp. In percussion you have no choice; something must yield,—either the stone or the instrument." He then states, "The instrument of the Danish surgeon, Jacobson, which acts by pressure, is preferable; and yet this instrument is scarcely applicable, but to small and friable calculi. I persist," he says, "in my objections against the instrument, on account of, 1st, The difficulty of properly seizing and fixing the stone. 2nd. If the stone be placed obliquely, with respect to the branches, the anterior moveable branch is liable to become deviated. 3rd. Even supposing the stone to be properly placed between the branches, the anterior branch is liable to be broken or bent by the shocks of the hammer. 4th. The stone, or its fragments, are made, by the shocks of the hammer, to fly from the branches of the instrument with a degree of violence which may often dangerously contuse or wound the bladder."

It does appear to me very inconsistent in Mr. Costello that after setting forth these remarkable objections on the 15th June, he should tell us, a month afterwards, that he had adopted an instrument acting upon the same principle, viz., percussion. Surely the profession had a right to expect, if Mr. Costello had been sincere in his objections, that he would have adopted the screw, or at least that he would have chosen gradual pressure for the destruction of the stone. Very surprising indeed is it that Mr. Costello should have sent his instrument to the same academy in the hope of obtaining the prize for it.

Theses and other remarks of a similar tendency (which would occupy too much space for insertion) formed the subject of that which I was prepared to address to the society on Saturday last had I been allowed to do so.

I hope my object will not now be misunderstood, particularly as I must disclaim everything like personal hostility or acrimony towards Mr. Costello. I consider that such inconsistencies as I have adverted to ought not to have been passed over by an institution like the Westminster Medical Society.

I remain, Sir,

Your obedient servant,

GEORGE WALKER.

209, Piccadilly, Dec. 11, 1832.

MEDICO-CHIRURGICAL SOCIETY.

We resume our notice and analysis of the the papers contained in the lately published volume (17th) of this Society.

FRACTURE OF THE LEFT FEMUR FROM DISEASE; THE BLADDER IN A STATE OF CARCINOMATOUS ULCERATION.

The paper of the President is followed by one by Professor Samuel Cooper, of the London University, containing some observations upon a case in which the left femur was fractured in consequence of disease, the bladder being at the same time in a state of carcinomatous ulceration. The following is a *condensation* of the facts:—

In the month of November, 1831, I was called on to visit C. Askey, aged 63, a coachman in Montague Mews, Russell-square, who had been for several months afflicted with severe pain in the left hip, pelvis, and loins; continual uneasiness in the glans penis; and frequent inclination to make water, which came away with difficulty, and sometimes blended with blood.

About half an hour before I saw the patient, a loud snap was suddenly heard in one of his limbs, as he was turning in bed, and he was immediately seized with excruciating pain in the left thigh. I now distinctly felt a crepitus below the trochanters; the limb was retracted; and it was manifest, that the broken part of the bone was surrounded by a swelling, characterized by a remarkable degree of hardness.

Long sufferings and frequent returns of profuse hemorrhage from the bladder had already reduced the patient to extreme emaciation and debility. The pulse was 130, irregular, and feeble; and the appetite entirely gone. Under these circumstances I applied splints to the broken femur, without any expectation of amendment of the health, or cure of the fracture. That the bleeding, and disturbance of the urinary organs, were not produced by a calculus in the bladder, was certain, because the quantity of blood in the urine was too copious to proceed from such a cause; the hemorrhage and pain had never been increased by exercise, nor even by the employment of driving a carriage; the sound, also, had been introduced into the bladder several times without detecting any calculus in it. After I had attended the patient a few days, the bleeding ceased, and the urine became quite natural and transparent; but a profuse hemorrhage commenced again on Sunday, November 20th, and he expired at ten o'clock the same night.

On opening the abdomen, a particularly hard mass presented itself on the fundus of the bladder, directly behind the os pubis—so hard, that the gentleman who first examined it with his fingers, supposed it to be a calculus. This indurated substance was found to constitute the base of a considerable fungus, from which the copious bleedings had taken place from time to time during the last five months of the patient's life.

When the chest was examined, the fifth rib on the right side was found to have given way near its central part, the ends of the broken portion being at the same time involved in a scirrhous substance, which had quite as indurated a feel as the hardest kind of cartilage.

The femur was surrounded at its broken part with a mass of the same kind of morbid tissue already specified, which here formed a thick protuberance. The consistence of this tumour was rather softer towards the bone, than more superficially, but the greater portion of it was peculiarly firm, and thickly interspersed with innumerable fragments of osseous matter.

As the patient had never complained of pain in the situation of the diseased and fractured rib, the discovery of such mischief was completely unexpected.

All the absorbent glands in the neighbourhood of the abdominal aorta, were enlarged (some of them to the size of a pigeon's egg), and converted into the same kind of scirrhous substance observed around the fractured bones.

Some differences of opinion may exist about what name ought to be given to the tumour connected with the femur: the disease could not be regarded as fungus hæmatodes or medullary sarcoma, the hardness of the morbid tissue being too great, and the general intermixture of osseous matter with it, constituting perhaps another difference. Besides, if it had been of this nature, the disease of the bladder would doubtless have been of a corresponding character; but this was not the case, for it did not form a large projecting mass, as happened in some other well-authenticated examples of fungus hæmatodes of the bladder, and especially in that published by my friend Mr. Langstaff in the eighth volume of the Transactions of this Society, p. 279. Neither were the fungous granulations connected with any soft, medullary mass; but situated upon a particularly hard substance, which constituted their base. According to my views, this affection of the bladder must be deemed cancerous; and if this inference be correct, may we not conclude, that the tumour which

grew round the femur was of a similar nature; for it can hardly be doubted, that the various forms of disease in this patient had a common origin from some peculiarity of constitution with which they were essentially united. The fact of the absorbent glands in front of the spine, from the pelvis to the diaphragm, being all in a completely scirrhous state, seems to corroborate these observations.

The indurated mass surrounding the broken femur, was not continued from the medullary texture, nor in any way connected with it. Yet, this texture, when a longitudinal section was made of the bone, was found altered from its natural appearance, even as high up as the cancelli of the head of the femur, and also to the extent of two inches below the fracture. Throughout all this track, it was much paler than the healthy continuation of it lower down, and filled with pus. The boundary line, between the healthy and diseased portions of the medullary texture, is still completely manifest in the preparations now in my possession.

STYPTICS IN ARTERIAL HEMORRHAGE.

Our readers will remember that some months back, Messrs. Talrich and Halmagrand made several experiments at the different metropolitan hospitals to demonstrate the efficacy of a "new styptic" in controlling hemorrhage. Mr. Cæsar Hawkins had been led to imagine that the theory of the arrest of hemorrhage, derived from the experiments of Dr. Jones, would account for the check which hemorrhage received in several of the experiments made by the French surgeons, and he instituted with this view a series of experiments, which, together with their results, are detailed in a paper, entitled *Account of some Experiments on the use of Styptics in Hemorrhage from Arteries.*

In most of Dr. Jones's experiments the free passage of the blood from the wounded artery was nearly, or completely, prevented by a continued suture in the skin. The mode in which, on the other hand, M. Halmagrand conducts his experiments is, to place three small tompons, or compresses of cotton, dipped in his "Liquide Hæmastatique," upon the wound of the artery, and retain them there for ten minutes with the fingers, after which a *single ligature* only, through the skin, is employed, which does not therefore close the wound, but merely prevents the compresses from being thrown out of the wound by muscular effort. This method I exactly imitated in the following experiments.

Experiment I.—I exposed the carotid artery of a sheep, and made in it a longitudinal wound about half an inch in length, and placed upon the orifice compresses of tow, of the same size as those which I had seen employed by M. Halmagrand, dipped in a decoction of *Aleppo galls*, and tied a single ligature in the skin before I withdrew my fingers from the wound. The compresses were taken away on the second day, and the sheep was killed on the tenth day by another experiment.

The wound in the artery was perfectly closed by lymph, without the formation of any coagulum in the vessel, which consequently remained pervious, the cicatrix itself being hardly perceptible.

Experiment II.—The same day M. Halmagrand laid bare the carotid artery of another sheep, so that the external wound might be such as he was accustomed to make. I then divided the artery transversely, to exactly half its circumference. The animal lost much blood, and fainted. When it revived, a short time afterwards, the artery again began to bleed freely, so that the syncope had not allowed of coagulation, and the animal would probably have died if nothing further had been done. I then placed on the wound of the artery the usual compresses dipped in *Ruspini's styptic*, and held them for ten minutes, using a single ligature in the skin. No hemorrhage occurred; the compresses were removed at the same time as in the first sheep, and the animal was killed on the eighth day. The external wound, which was suppurating, communicated directly with the artery, which was completely blocked up by a firm coagulum, united by lymph to the interior of the vessel for about half an inch above and below the opening, connected on the side next to the heart by a loose coagulum two inches long.

Experiment III.—I exposed the carotid artery of a sheep and cut out a circular portion of the vessel, a kind of wound which Dr. Jones had not tried (probably because it is not one that is likely to occur in practice), and placed over the opening the usual compresses dipped in *plain water only*. The animal was so restless, however, that I could not retain my fingers on the artery, so as to prevent some escape of blood, which was renewed as soon as my pressure was relaxed, and consequently the animal would probably have died in the same way as another sheep in which M. Halmagrand had made a similar, but somewhat smaller, wound, and had afterwards applied his styptic unsuccessfully. I therefore altered my experiment, and united the skin coarsely by a *continued suture*, so as nearly to confine the blood in the same manner as Dr. Jones had done in his experiments. The

integuments were distended with blood as soon as my fingers were removed, and some escaped also between the stitches, which were loosely fastened. The animal got up, however, and there was no further hemorrhage; it was killed on the sixth day, when the wound was open and suppurating freely. The interior of the vessel was completely filled with coagulum to a considerable distance above and below the wound, which was blocked up by a portion of the coagulum projecting into the aperture. (See preparation No. 3.)

In the fourth experiment, where the wound of the soft parts was small and direct, the animal died of hemorrhage on the third day.

From these and other experiments, Mr. Hawkins says it would appear "that out of *four* experiments performed in the same manner as M. Halmagrand's, but without the use of his styptic, *two*, viz., Experiments 1 and 2 were permanently successful, in one of them the artery being left pervious, in the other being obliterated by coagulum; that a third, Experiment 3, failed at the time of the operation for want of sufficient coagulum in the vessel to defend it after the compresses were loosened; and further, that Dr. Jones's method saved this animal after simple pressure had not succeeded; and that one only, viz. Experiment 4, was altogether unsuccessful, the animal having in this case only died of the operation.

The conclusions which may be drawn from the experiments of M. Halmagrand and Mr. Hawkins are the following:—

1st. Experiments 1, 2, and 4, demonstrate that pressure continued for ten minutes upon an injured artery, *in a nearly open wound* of the soft parts, by means of small compresses moistened in various liquids, may check the hemorrhage from an artery in which a longitudinal or transverse wound has been made, or from which a portion has been removed.

2ndly. Experiments 3 and 5 show that when a portion of an artery has been *altogether removed*, the hemorrhage may be restrained, and the vessel permanently closed, by allowing the blood to coagulate in the soft parts, as in a diffused aneurism.

3rdly. Experiments 1, 2, and 4, prove that the means by which hemorrhage is controlled, when pressure is made for a short time in an *open wound*, are the same which Dr. Jones's experiments had shown to be exerted in a *closed wound*.

4thly. It is shown further, that if coagulation takes place imperfectly, or if the clot is suddenly or violently disturbed, immediate hemorrhage will occur.

5thly. Experiments 4 and 5 show, that although the hemorrhage may be controlled for a time, it may subsequently return at various periods from the causes pointed out in the last section as capable of producing immediate hemorrhage.

6thly. It is probable that styptics applied to a bleeding artery can only be beneficial in two ways—first, by causing contraction of the coats of the bleeding artery, or, secondly, styptics may be of service by accelerating or promoting the coagulation of the blood. If, for instance, the decoction of galls, which was employed in the first experiment, be mixed with recent blood, it instantly produces coagulation, and such, I am informed, is also the effect of MM. Talrich and Halmagrand's "Liquide Hæmostatique;" but Ruspini's styptic, which was permanently successful in the second experiment, and which is often employed in practice with marked benefit, produces no coagulation, nor any other sensible effect on recent blood. On the whole, we are justified in concluding that if styptics *are* employed, the cessation of the hemorrhage is to be ascribed principally, if not entirely, to the pressure, or at all events in a minor degree only to the action of the styptic.

7thly. These experiments further demonstrate that the effects of pressure, with or without the use of styptics, are precarious and uncertain, even in brutes, and therefore, *à fortiori*, ought not to be depended on in cases of wounded arteries in man, to the exclusion of the almost uniform success of the ligature, whenever its employment is practicable.

THE VENEREAL HOSPITAL,

PARIS.

There is no hospital in Paris where more valuable and practical information is communicated on any special branch of therapeutics than at this. M. Ricord, the visiting surgeon, is distinguished for his industry and ability, for the nicety of his diagnosis, and the judicious treatment he employs. The hospital contains several hundred beds, which are always tenanted, and there is moreover an out-door dispensary, at which multitudes attend every week. The student is thus supplied with the most extensive opportunities of studying the maladies originating from diseased intercourse, in all their degrees of intensity and variety. There are, moreover, two spacious wards devoted to the reception of children tainted with syphilitic diseases. M. Ri-

cord's visit is a bed-side clinical lecture of the most valuable kind. We shall occasionally afford our readers an outline of his opinions and of the results of his practice.

NATURE AND DIAGNOSIS OF BLENNORRHAGIA (OR GONORRHŒA).

Blennorrhagia, according to M. Ricord, is an affection which may originate in various parts, separately or collectively, of the genito-urinary apparatus. It may occupy the vulva alone, that is, the part of it which is situated before the caruncule myrtiformes. Its seat may be the vagina exclusively, and isolated points of that conduit may be alone affected. Vaginal blennorrhagia, then, is sometimes furnished by a single parcel of follicles. When thus partial, it often affects the vaginal cul-de-sac, which surrounds the neck of the uterus, and the parts discovered by the speculum present the aspect of the prepuce and gland in the disease denominated *balanite*. Blennorrhagia may also derive its source from *the womb alone*. Brugnone has already pointed out this fact, and M. Ricord's experience fully attests his accuracy. It may also be exclusively urethral, contrary to the opinion of Swediaur; but this affection is comparatively rare. The most common is the urethro-genital discharge, having its seat at the same time in the urethra and vagina, or in the urethra, vulva, and uterus, together.

The secretions furnished are mucous, simple, serous, sero-sanguinolent, sero-purulent, milky, caseous, sebaceous, like that found between the gland and the prepuce; purulent, mucous, and opaline, or mucous and transparent. They may be glairy or more or less fluid.

The parts affected are sometimes red, hot, and shining alone. This is the erysipelatous inflammation of Fabre, and may occasion no discharge. The mucous membranes of the genital parts is then in the same condition as the conjunctiva in dry ophthalmia; but generally the mucous membrane is covered with one of the secretions we have described, and then the epithelium may be deficient in the point which furnishes the secretion, and which takes on the appearance of a burned or blistered surface. Ulcerations of various kinds are in other cases the source of discharges of which some appear to be of a consecutive character. At other times, again, a follicular eruption, more or less prominent, is observed, giving a papulous or granular relief on the surface, like that which in the intestine has lately been termed *psorenterite*. Finally, isolated or confluent mucous pustules, and excrescences of various kinds, may occur in the parts from whence the discharge emanates, and these, it should be remarked, are frequently met with in persons who deny any impure connexion.

Neither the aspect of the secretions, nor their particular seat on which Graaff used to establish his diagnosis, can serve as a pathognomonic sign, *in the cases in which there is no characteristic lesion of tissue*. The pretended intermission in fluor albus admitted by Baglioi as a distinguishing sign between that disease and gonorrhœa, has no existence, for in a uterine or even vaginal blennorrhagia, the catamenial flux mingled with the venereal discharge, will disguise it in the same manner that it marks leucorrhœa in a great number of cases. As to the point wherein the disease commences, the experience afforded by more than 600 patients, showed it to be in the vagina; the cases in which it commenced in the urethra were clearly exceptions, as might be conceived from the relation of the genital parts during the communication of the infection. Nevertheless it is true, that although a discharge from the urethra may depend on other causes, still when this discharge accompanies one from the vulva, uterus, or vagina, there are strong reasons for supposing it to arise from infection. As to the pain whether in passing urine or otherwise, it affords no characteristic sign, and is often wanting in very acute and severe discharges. The diagnosis, therefore, remains obscure. Experiments by inoculation are now in progress on infected females, the results of which will probably elucidate the question.

It is important to observe with attention the progress of blennorrhagies either to a cure or to the chronic state. The vulva is generally the first part cured; then the anterior parts of the vagina; then the urethra, then the deep-seated parts, the utero-vaginal cul-de-sac,—the mouth of the uterus and the uterine cavity itself. These facts can be verified every day on masses of patients by the pupils who attend the wards. The more rapid cure of the parts exposed to the air, or nearer its contact, is incontestable. It suggested even the idea of making the patients wear small specula of gum elastic, when the inflammation was sufficiently abated. From the preceding observations it follows, that *in the chronic state* the deep-seated parts are those concerned in the majority of instances, whether that they were originally and exclusively concerned at the commencement, or that the other parts had been cured in the order pointed out.

The deep-seated blennorrhagiæ, those the most frequently with lesion of tissue, and always contagious, are of the highest importance to understand. Women thus

affected have usually all the external characters of health; it is only the speculum which discovers the true source of the disease.

To the preceding note of M. Ricord's observations it may not be superfluous to add the following remarks on the same subject by M. Tanchou, in a paper recently read before the *Society de Médecin Pratique*, on the diseases of the neck of the uterus and on leucorrhœa. Relatively to the latter, the use of the speculum appears of great value in recognising the state of the vagina, and determining the nature of the discharges, which vary according to their source. First, there may be a white liquid discharge of serum, apparently the product of augmented secretion of the mucous membrane, and having its seat close to the orifice of the vagina; secondly, a white, thick, creamy fluid, proceeding from the upper part of the vagina, and especially from the mouth of the uterus, frequently accompanied by aphthous ulcerations, surrounded by a red areola; thirdly, a white albuminous opaline matter, analogous to that remarked during labour, when the mouth of the uterus commences to dilate; a secretion having its seat, not in the body, but in the neck of the uterus, in a multitude of concentric folds, which form a kind of sphincter to that organ. M. Tanchou further dwelt on an affection of the stomach frequently associated with leucorrhœa. When this occurs, the discharge becomes of a mixed consistence, of a colour varying from white to green or yellow. As to the diagnosis between gonorrhœa and leucorrhœa, he considered it quite practicable. In gonorrhœa, inflammation more or less acute occupies the vulva, the vagina, and even the urethra, which never takes place in leucorrhœa. Examination by the speculum discovers a great quantity of sebaceous glands swollen and inflamed, discharging a purulent fluid of peculiar odour; when the gonorrhœa is of longer duration, it leaves at the entrance of the vagina spots or stains like ecchymoses, sufficient to enable us to distinguish the seat, causes, and intensity, of the two diseases.

A still more marked mode of diagnosis was pointed out by M. Moncourier, who asserted that leucorrhœa ceases during the menstrual flux, while, on the other hand, gonorrhœa continues. Of the accuracy of the latter statement, there is abundant proof at this moment in M. Ricord's wards, but it is very desirable that the question of the cessation of leucorrhœa during menstruation should be submitted to further examination.

ST. BARTHOLOMEW'S HOSPITAL.

HÆMATEMESIS.

Charles Bowers, ætat. 30, was admitted into John's Ward, Nov. 15, under the care of Dr. Latham.

The patient states that he has been for many years an invalid, enjoying only intervals of comparative ease and health, but the immediate object of annoyance is a vomiting of blood which has lately supervened. The foundation of his bad health he considers to have been laid in India some years back, when he caught a severe cold from sleeping on the ground exposed to the heavy dew, and from thenceforth he has been the subject of continual attacks of one kind and another. Having travelled, however, as a sailor to all parts of the globe, he confesses to have been a "great drinker." During the last few months, œdema of the lower extremities has come on, with a general increase of indisposition, so as to oblige him to discontinue his work as a labourer. Depression of spirits, and progressive loss of appetite and of strength, were still further aggravated by vomiting of blood. This latter symptom first made its appearance a week ago. The ejected matter was profuse, and the stools at the same time became black and bloody. This state of things has continued with variable but frequent intervals, the last vomiting having occurred during the past night. The countenance is full of anxiety and depression, and, like the whole surface of the body, sallow and bloodless. Lividity of the lips and gums. He complains of shivering. The legs are swollen, the hands moist and cold. No pain manifested on pressing the abdomen, the feel of which is natural, with the exception of a defined hardness stretching from the right hypochondrium across the scrobiculus cordis. Tongue moist and pale; pulse 120, soft.

Dr. Latham, in accordance with his usual custom, made some remarks. He observed that this was an interesting case to watch,—interesting not only in regard to medical improvement, but because the successful treatment of such cases usually conferred a great degree of reputation on the practitioner. In the eyes of non-professional persons, the vomiting of blood, with its concomitants, always bore such a formidable aspect, that the skilful use of remedial means seemed to them to confer a magical power. It was astonishing what slight causes sometimes gave rise to hæmatemesis. This was well illustrated in the instance of a fashionable lady whom he attended, in whom, though apparently in good previous health, it had come on with constipation, after the exertion of a journey, and was

quickly removed by regulation of the bowels. Look at that patient: a superficial observer might think him marked for death, yet perhaps there was little doubt of sending him out cured. But how cured? Only of the present hæmatemesis, for there was evidently much *organic* disease. The man had been a dram drinker, and his liver was in the disordered state to which such an abuse gave rise. The only indication of treatment here was, to endeavour to get rid of the hæmatemesis by acting on the intestinal tube. With regard to the melæna, the black stools were half-digested blood, most probably escaped from the stomach, or perhaps partially thrown out from the coats of the intestines.

Ordered *calomel* gr. ij, every other hour for six hours, and after three hours, *sulph. magnesiæ* ʒj ex aq. menth.; *catapl. sinap.* to the epigastrium.

A fit of the vomiting came on whilst we were in the ward. The ejected matter, in quantity about a pint, consisted of blood, mixed with the common contents of the stomach, and emitting a very peculiar odour.

16. The calomel was taken, and the dose followed an hour after, by the hæmatemesis and black purges. The patient expresses a feeling of ease and lightness, which he says has usually been the case after these evacuations of the stomach. *Calomel* as before.

On the 17th he was ordered *castor oil*, which he continued to take as required, till the 19th, when great improvement was apparent. There had been no recurrence of the hæmatemesis since the period of the first note, and the motions had their proper character. Pulse frequent and weak; tongue natural, excepting its paleness. *Perget.*

Here Dr. Latham was induced to hope that a temporary cure had been effected. The secretions, he said, were restored, and the man would probably go out, but his liver would assuredly sooner or later bring him back again. However, it was to be noticed that what had been accomplished was solely by the simple action of *purgatives*. Noticing the modes of relief adopted by nature in cases of diseased liver, Dr. Latham said, that *here* the stomach was pouring forth blood. In the case of the predecessor* in the same bed, the same object had been attempted by, he might call it, a "dropsical *seizure*," which, seemingly insufficient, was followed by a hemorrhage from the nose; whilst, on examining the body of Mary Cook (another liver case), a peculiar method of relief was observed, for the circulation of the liver being interrupted,—in addition to large effusion, a new set of bloodvessels had been established.† With regard to the patient Bowers, there was every probability that he would become the subject of dropsy.

The patient, after this, continued much the same, without any symptoms requiring particular notice, till the evening of Friday the 23rd, when he relapsed into a state of great prostration, complaining of nausea, with the pulse 120, very feeble, and the tongue dry. He sunk rapidly, and died on the Saturday night.

Sectio Cadaveris 36 Hours after Death.

Rigidity of the limbs; surface of the body very pallid. On dividing the integuments, a corresponding bloodless state observable in the flabby muscles.

Abdomen.—A quantity of straw-coloured serum in the cavity of the peritoneum. The stomach, free from blood, contained a greenish fluid, with some mucus, amounting to about a pint. General paleness of the internal coat of this organ, particularly around the pyloric extremity, but at the cardiac orifice the veins were full; no alteration of the mucous membrane was apparent; no traces of blood throughout the entire intestinal tube; the small intestines contained a yellow mucous fluid, assuming the character of fæces towards the rectum. The liver, very largely and firmly adherent to the diaphragm, and to the right kidney, extended low into the left hypochondrium. Externally it presented large unequal prominences, and its peritoneal investment was thickened; internally were observed fibrous striæ traversing its substance, which exhibited a state of complete anæmia, and was of greater density than natural. The coats of the gall-bladder were thickened, and contained a little light-coloured bile. The peritoneum investing the spleen presented patches of a cartilaginous appearance. The organ itself was very much enlarged, its substance easily breaking down into a pulp, resembling light chocolate paste. Kidneys large, highly injected, but without any morbid change.

Thorax. Lungs healthy. About three ounces of turbid serum in the pericardium, with small flakes of lymph floating in it. Muscular structure of the heart pale, flaccid, and easily lacerable. Right auricle filled with a yellowish jelly-like coagulum. Enlargement of the auriculo-ventricular open-

* An old attendant at the Corporation dinners, with his "ruling passion" so "strong in death," that he even managed to secrete a gin bottle under his bedding. He was tapped for ascites, and quickly died. On the *postmortem* the hepatic disorganisation was found to be very great.

† M. C. died of *ascites*. Her liver was diminished to less than one half its natural size, and Mr. Kiernan on injecting it, discovered that a *collateral venous circulation had been established through the diaphragm.*

ing, and of the corresponding opening on the left side.

Head.—When the integuments were reflected from the skull, a very marked depression or shaving off appeared on the right parietal bone, without any corresponding irregularity on the inner table of the cranium, the wall of which was at this part almost transparent. No cicatrix on the scalp. The only other circumstance needful to remark here, is the general pale state of the brain and its membranes. There were firm adhesions of the dura mater.

Among some interesting remarks which Dr. Latham offered on this *post mortem*, he observed that one would have expected from the suddenness of the relapse to have found at least a small quantity of blood effused into the stomach, which, after such drains on the constitution, would have sufficiently accounted for the immediate cause of death. However, these were some of the almost inscrutable accidents of life, and in illustration he mentioned two or three cases of sudden death from no *apparent* cause, where not the slightest trace of disorganisation was recognised.

FEVER.—ERYSIPELAS.

Vincent Rose, ætat. 48, admitted Nov. 15. When first admitted, seemed to be labouring under simple continued fever, and in consequence of his strange bewildered manner, no substantial account was obtained of or from him until the second day after his arrival. He stood shivering, looking pale, dusky, and anxious, with a hurried wandering in his manner, interrupted by short comparatively lucid intervals. He stated, first, one thing, then another, and on being touched in many parts, shrunk with an expression of pain. It was gathered from him that he was a waiter, and that whilst attending in that capacity on Lord Mayor's day, he was seized with vertigo, pains in the joints and head, and alternate shivering and "feverishness." He was now giddy; had a slight cough; was continually muttering. Eyes much sunk, sometimes fixed, and expressive of great anxiety. To these symptoms were joined a very feeble and frequent pulse, and a dirty state of tongue. Thirst; no appetite; bowels opened by medicine.

16. Delirious during the night. Very little sleep. Still talking to himself, and complaining of pain around the left axillary; pulse 80, with more power. Ordered *calomel* gr. ij, to be repeated in four hours. This is the first medicine he has had in the hospital.

17. Has passed a better night. Yesterday attention being called to the axilla, it was observed that there was an erysipelatous redness on the left pectoral muscle. It was ascertained from his medical attendant that the patient's account of the attack was correct, and, in addition, some interesting circumstances were mentioned, illustrating the contagion of erysipelas. It appears that Rose had been nursing a man who laboured under erysipelas during the nights of the 7th and 8th instant, and on the following day he himself became the subject of it, the affection appearing on the breast and back. His wife moreover is a sufferer from the same complaint in the head and face, which has commenced since her husband's indisposition. He has had two liquid stools. The side is to be poulticed.

18. Had a comfortable sleep last night, and appears more easy. Bowels open; a cleaner tongue; pulse 98.

19. Extension of the erysipelas towards the hip. Not so well.

Carb. ammoniæ gr. ij,

in an ounce of distilled water every other hour.

20. The erysipelas assumes an erratic character. He now complains of great uneasiness about the left knee. There is swelling around it, and also around the left metacarpus; tongue dry; pulse 100, hard and full.

Calomel;
Pulv. Jacobi aa gr. ij,

to be taken every sixth hour.

21. The joints still swollen and painful; erysipelas stationary. There are some small miliary vesicles all over the body; tongue dirty; pulse 112; very low, with difficulty of breathing. To have eight leeches to the knee. Omit the *cal.* and *p. jacob.* He is to take *pulv. ipecac. comp.* gr. v, every sixth hour.

22. No material change in his condition; tongue dry; pulse 120, and strong; skin moist. More leeches to the knee, which is not so much swollen.

23. Some improvement; the pains have forsaken every part but the knee, to which the leeches are to be repeated. *P. ipecac.* gr. x, every night, and omit preceding order.

27. The man has been in a wavering condition up to this day. There is now a decided change of aspect for the worse. The nurse says that he was worse last night than he has yet been; he was very delirious, and passed his motions apparently involuntarily. There is also sloughing of the integuments covering the sacrum, and on the back; tongue dry and ragged; pulse 120; the Dover's powder has been omitted. *Carb. ammon.* gr. ij, *ex aq. distill.* Four ounces of wine.

29. He has passed the last two nights in a restless manner, completely dilirious. The contents of the bladder and rectum

pass beneath him. Collections of matter formed in the right arm and hand. Two of these were opened, and discharged "laudable" pus; tongue dry in the centre, but moist and white at the edges.

30. He lingered through to-day, supported by some laudanum, and died during the night, after much delirious muttering and groaning.

Autopsy twelve hours post-mortem.

On examining the body, various collections of white thick pus were found in various parts. The largest was formed under the left pectoralis major, extending under the whole of that muscle, and, as some supposed, communicating with the shoulder-joint of the same side. The synovial membrane of the left knee-joint was distended with white pus, and the same kind of matter penetrated down the right arm along the extensor tendons of the thumb. The cartilages of the affected joints were softened, particularly in the knee, the capsule of which was injected. The great veins of the superior and inferior extremities were minutely examined, and found to be quite healthy.

On opening the pericardium, which was much distended, there escaped a quantity of turbid serum with flakes of lymph in it. The surface of the heart presented a well-marked specimen of the morbid appearances of pericarditis. It was covered with a thick coat of lymph, easily removed, rough, and laminated. The interior of the organ was not inspected. The surface of the loose pericardium was lined with the same coating, bands of the same character passing from it to the heart. The apex of the right lung contained a few tubercles in an early stage. The left lung was healthy, but the opposing sides of the pleura were covered with a layer of recent lymph, and adherent in some points. There was also some effusion of serum in this part. Nothing particular in the abdomen, with the exception of there being three supernumerary spleens, about the size of nutmegs. There was an effusion of fluid beneath the arachnoid, elevating that membrane at its convexity, but the substance of the brain was natural.

THE CAUDAL HEART IN THE EEL AND CONGER.

To the Editor of THE LANCET.

SIR,—Observing in your memoir of Dr. Marshall Hall's essay on the "Circulation of the Blood," that you state the part described by him in the tail of an eel, under the name of a caudal heart or ventricle, to have been before described by Professor Jacobson of Copenhagen, I take the liberty of pointing it out to you as an error, and of referring you to the 19th volume of the *Edinburgh Medical and Surgical Journal*, where Professor Jacobson's researches are reviewed. What is there stated is, that "in other vertebrated animals than the mammalia," a new and peculiar system of veins exists, which is not directly united with the other veins of the body. By means of the veins which compose this system, the blood, which flows back from the middle or posterior parts of the body, does not go directly to the vena cava inferior, and afterwards to the heart, but is conveyed either to the kidneys or to the kidneys and liver. This system is observed in birds, reptiles, and fishes, and its primary form undergoes three degrees of modification." This venous system "is destined to convey the venous blood returning from the middle or posterior parts of the body to the kidneys, or to the kidneys and liver, and regulates in these organs the functions of secretion. Hence in birds, reptiles, and fishes, the secretion which takes place in the kidneys, is effected by means of veins and venous blood." This you will see is very different from the part described by Dr. Hall, and exists in a large class of animals, whereas the caudal heart exists, as far as his inquiries have gone, only in the eel and conger, and is not found in a fish so nearly allied to these in form as the lamprey. What it actually is, or what is its use, seems difficult to determine, but I believe Dr. Hall is the first who has noticed the structure. Having assisted him in the whole of the investigations preparatory to his essay, and witnessed every fact recorded in the work, I may be excused for addressing you on the subject, more particularly as the same mistake of a similarity of structure occurs in the review of Dr. Hall's essay in the Edinburgh Medical and Surgical Journal for January, and where even the reference is given to Professor Jacobson's description in the 19th volume of the journal.

I remain, Sir, your obedient servant,

HENRY SMITH.

Torrington Square, Dec. 7, 1832.

CORRESPONDENTS.

As we find it impossible to insert replies in this week's LANCET to *all* the questions of our several correspondents, we think it prudent, in order to avoid an appearance of partiality, to defer the insertion of the whole of the notices prepared for this page, until our ensuing Number.